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PHYSIOLOGICAL ABSORPTION.

A PLAIN mechanical theory for a process occurring within a living organism is rightly dear to the heart of the modern physiologist. The hypotheses of such a theory are grasped with great clearness by the mind, a clearness enhanced, too, by contrast with the dark background of mysticism formed by the past metaphysical conceptions of Stahl and Whytt. We attempt to explain the phenomena of life on the basis of the knowledge gained by the physicist and chemist, but we essay a hard task ; for if he who deals with inanimate nature must often admit that the conditions under which he works are obscure, how much more is this the case in processes carried out in that tangle of interactions, the living cell !

Can we then feel surprised that the regeneration of physiology that has been so evident since the time of Liebig should, in its over-confidence, have somewhat overstepped the mark, and that in calmer mood we are now becoming convinced that until a clearer insight into the mechanics of the cell has been gained, we cannot hope to grasp the rationale of the simplest processes of the economy ?

Physiology is in no real danger of again succumbing to the mysticism of "vitalism," and Rindfleisch's expression, "neo-vitalismus," is certainly unfortunate, but physiology

is recognising that, until the factors of that bald, but as yet necessary, expression "Vital Action" can be analysed and harmonised with physical forces, little progress can be made towards an intimate knowledge of the essence of life.

The history of the development of our ideas concerning the absorption of nutriment is an interesting one, as indicating how ignorant we as yet are of much that is concerned in a process which until recently was thought to be explicable upon purely "physical" grounds.

The classical work of Dutrochet, published at the close of the third decade of the century, was hailed soon after by physiologists as giving an indication of the lines upon which the movements of fluids through membranes in the living animal could be explained. It appeared to be beyond doubt that osmotic action would alone suffice to account for the phenomena of absorption so far as they had then been studied.

Yet, as will be evident from the sequel, we have now to admit that, though osmotic action is a factor, and often of great importance, experiment shows that another factor has to be considered, *viz.*, the living condition of the cells of the membrane through which the solutions pass.

A glance at our present knowledge of the main anatomical points in the structure of the intestinal membrane must precede our physiological considerations.

A multitude of villous processes are clad with specialised columnar cells, whose inner blunt ends rest upon the parenchyma of the organ somewhat condensed at its periphery. A network of blood capillaries is in many places in immediate contact with the ends of these cells, and in the depths of the parenchyma (a more marked structure in carnivors than in herbivors) is imbedded a lacteal tube with blind end and distinct endothelial wall. The epithelial cells are but loosely attached at their inner ends, though cemented together at their sides, and their renewal according to Bizzozero, Heidenhain, Cloetta, and Schaffer is effected from below, from the cells of the crypts of Lieberkuhn, which divide in a plane at right angles to the long axis

of the crypt, and push the new cells upwards. The cells themselves are pliant, easily accommodating their shape to the changes of that of the villus wrought by its specific muscle, and possess at their free border the well-known "basal band" (*basalsaum*) of Kölliker, with the "rodlet organ" (*stäbchenorgan*) of Brettauer and Steinach.

This organ to which so much attention has been devoted is, according to Heidenhain, composed of a number of minute rods in intimate connection with the cell protoplasm and imbedded in a somewhat softer material. The rods can be retracted into the cell, and are also capable of considerable extension beyond it, becoming hair-like, a condition most easily brought about by direct excitation of the surface either chemically or mechanically (*e.g.*, by tape-worms in the gut). As confirmatory of this idea of the rodlet organ must be mentioned the recent work of Miss Greenwood, who in the earth-worm saw appearances suggestive of "retractile cilia" in the gut cells, those cells which are loaded with nutriment having retracted cilia, while those devoid of food stuffs have their cilia extended. Brettauer and Steinach, indeed, thought that the rods were present in the basal band during hunger but absent during digestion.

Thanhoffer, too, has maintained that in the frog, especially when the mucosa is wetted with bile, processes extend from the basal band in the fasting state, and Wiedersheim maintains the existence of a similar state of affairs in the gut of the amphibian *Spelerpes fuscus.* The swallowing of food particles by means of pseudopodial processes in the digestive cells of certain lower Metazoa is known to us through the researches of Metschinkoff upon intracellular digestion, and Wiedersheim admits the possibility of some such action in vertebrates, though the ancestral method is modified, in that by the substitution of extra- for intracellular digestion the cells will only take up matter that has been especially elaborated for them by the digestive ferments. These observations, especially those of Thanhoffer in the frog, urgently need repetition.

The epithelium then of the villus is highly specialised, and, as far as the passage of fluid from the gut to the

blood is concerned, two ways are open, through the protoplasm of the cells themselves, or through the cement substance between these structures. The ubiquitous leucocyte is present in the fluid filling the fine connective tissue sponge work of the body of the villus, and may mount in the soft cement between the surface cells and gain the gut surface. Especially in hunger and hibernation do they pass between the epithelial cells. The oxyphil variety is very frequent, and is most numerous, and contains most granules during digestion. Large conglomerate macrophages also abound, holding the remains of effete cells and micro-organisms.

It will be well at first to confine our attention to the consideration of the absorption of water, and of substances soluble in water which occur in the intestine, *viz.*, salts, sugars, and albuminous bodies.

The older ideas of the absorption of fluids were Imbibition (Rudolphi and Magendie), Osmose (Dutrochet) and Filtration (Brücke), and the merit of first definitely doubting the sufficiency of the two latter processes, to explain the phenomena of absorption in the gut, is due, probably, to Hoppe Seyler.

Hoppe Seyler pointed out that filtration, under peristaltic pressure, and such pressure is never great, cannot be of much value in moving the contents of the gut through its walls, on account of the plasticity of the latter, and, moreover, against the osmotic theory he advanced the fact that weak alcohol in the gut causes no outpouring of water from the blood, but the whole solution is quickly absorbed. He considers that "Bewegungs-vorgänge in den Zellen" are the cause of absorption, and instances the choleraic patient with shed epithelium, whose absorption is at a standstill. It must, however, be stated that Johannes Müller had the idea of the action of the cell long before, for he speaks of it as exerting an "organische Anziehung".

We have many cases in which it has been shown that the diffusion of solutions does not take place with equal ease in living and dead membranes. Claude Bernard found curare passed with greater difficulty across the living than

across the dead gastric mucosa; Susini that the epithelium of the bladder interfered with the diffusion of potassium ferrocyanide to the water in which it was suspended, and Cazeneuve and Livon showed the same thing as regards urea. Fleischer, too, demonstrated that sodic salicylate diffused far more slowly through a living than a dead loop of intestine.

Further than this, it has long been known that diffusive currents may occur with greater ease in one direction than the other through certain animal membranes. As long ago as 1825, Lebküchner showed that solution of potassium ferrocyanide diffused more readily from within outwards than in the reverse direction through the intestine of cat, rabbit, and fox, and Matteucci and Cima found similar differences of osmotic transfer of water across the skins of the frog and eel, and certain gastric mucosa, according to the direction.

Such observations showed that if we are to follow the process of absorption of solutions from the intestine, we must proceed by direct experiment only, and not by deduction from results gained with dead membranes. Experiments have now definitely shown that in addition to the osmotic process, we have also phenomena that cannot be explained except upon the assumption that the living cells themselves exert a "triebkraft" during absorption. We have admitted such an assumption for the act of secretion since the days when Ludwig showed that the secretory pressure exceeds that of the blood, and for absorption, which is practically a reversed secretion, we must now make the same admission.

The data concerning osmose with which we are concerned are as follows:—

1. If the solutions on the two sides of a porous membrane have equal osmotic pressures, such solutions undergo no change of volume.

2. If solutions of unequal osmotic pressure are separated by a porous membrane, water passes from the side where there is a less to that where there is a greater osmotic pressure.

3. The total osmotic pressure of a mixture of dissolved substances is equal to the sum of the partial pressures of the several ingredients.

4. If solutions of equal total osmotic pressures but unequal partial pressures of the dissolved substances are separated by a porous membrane, any constituent of the mixture passes from the side at which it is present at higher partial pressure to the other side, until the partial pressures on the two sides are in equilibrium. No passage of water across the membrane, however, takes place.

If intestinal absorption depends upon osmosis, the facts of experiment must conform with the above laws.

In 1885 a series of researches was commenced in the Breslau laboratory under Heidenhain's direction with a view to deciding the question.

Leubuscher found that solutions at body temperature supplied to a loop of gut in a narcotised dog were absorbed, and better at a pressure of about 10 cm. of water than above or below; unfolding of the intestinal walls obviously at low pressure favours absorption by exposing more surface, while at high pressure compression of the blood-vessels of the mucosa interferes. His most important point was that ·25 to ·5 per cent. sodic chloride solutions were absorbed quicker than water. At 2 per cent. up to 10 per cent. a clear osmotic process was evident, for while the volume of fluid in the gut increased, the amount of sodic chloride fell. With such strong solutions, however, it is, of course, impossible to regard the condition of the epithelium as normal. Comparing the absorption of sodic and potassic chloride, it was found that the former is absorbed faster than the latter, though Graham showed that potash salts are the more diffusible of the two.

Gumilewsky, using a dog with a Vella fistula, confirmed Leubuscher's point regarding the fact that weak sodic chloride solutions are absorbed with greater rapidity than pure water, but added that the relative rates of absorption of water and salt in sodic chloride solutions varies with the concentration.

At a strength of about ·6 per cent., the salt and the

water are taken up *pari passu;* below this strength, the water is absorbed faster than the salt, while above, the salt leaves the gut faster than the water.

Röhmann, also using the dogs with Vella fistula, added some points against the pure osmotic theory. Peptone and sugar, in spite of their well-known differences of diffusibility, he found to be absorbed at about the same pace. Again, in a comparison of the diffusibilities of cane sugar and sodic sulphate, as determined by C. E. Hoffmann, for ox pericardium, with their rapidities of absorption in the living gut, he found that while the rapidity of diffusion of sodic sulphate slightly exceeds that of cane sugar (1·15 : 1), yet the absorption of cane sugar took place at about ten times the pace of that of the soda salt.

These experiments of Heidenhain's pupils had all demonstrated cases which were negative as regards absorption by pure osmose, but no cases of absorption under conditions excluding the possibility of osmose were brought forward. Such absorptive action was, however, shortly shown to be capable of demonstration by Waymouth Reid, who obtained a transfer of normal saline solution across the exsected gut mucosa of the rabbit under conditions of equality of osmotic pressure on the two sides of the membrane, and, moreover, obtained a reversal of the current by adding pilocarpine equally to the two masses of fluid on either side of the intestine.

Heidenhain himself has again quite recently published more experiments emphasising the necessity of considering the process of intestinal absorption as involving other factors besides those of differences of osmotic pressure between the contents of the gut and the blood of the capillaries of the villi. The observations deal with the absorption of solutions of salts, and especially of sodic chloride. For determining the total osmotic pressure of such complex fluids as the serum of the blood, Heidenhain has followed Dreser in choosing the method by estimation of the lowering of freezing point (Raoult), and used the Beckmann apparatus. The method is, perhaps, not so delicate with watery solutions as the blood corpuscle method of Ham-

burger, or even the plasmolytic method of De Vries, but is more applicable to coloured solutions, and is certainly accurate enough for the gross differences dealt with in most of the experiments.

The osmotic pressure of serum is, of course, in the main due to the relatively high partial pressure of the sodic chloride which bulks as the main salt of the ash, for the albuminous substances contribute but a small quota.

That there is such a thing as a physiological act of absorption is again urged by Heidenhain, by showing that if dog's serum of the same osmotic pressure as the serum of the experimental dog is placed in the gut, its water and salts are absorbed in the same ratio as they have in the original fluid, so that the osmotic pressure of the residue in the gut, if the absorption be interrupted, is little different from that existing at the commencement of the experiment; the albumin is absorbed more slowly than the salt, a fact known long ago to C. Voit, but as mentioned above, the albumin contributes but little to the total osmotic pressure of the solution. In one case, serum with $\Delta = \cdot617$[1] was placed for two hours in the loop of the gut of a dog whose serum had $\Delta = \cdot626$. At the end of this period, 42 per cent. of the water and 40 per cent. of the salt had been absorbed, and in the fluid remaining in the gut it was found that $\Delta = \cdot600$.

The same kind of absorption must also be possible for solutions of sodic chloride of the same percentage as the blood; and it will be remembered that Gumilewsky had shown this to be the fact, *i.e.*, that with solutions in the region of ·6 to ·7 per cent. the ratio of salt absorption to water absorption is expressed by unity. Gumilewsky also showed that with solutions of sodic chloride of higher percentage than the blood (since the salt passed into the blood faster than the water), the ratio of salt to water absorption is greater than unity; while, on the other hand, with the solutions of lower percentage than the blood, the water

[1] The symbol Δ is used to express the lowering of the freezing point of a solution, and is proportional to its osmotic pressure.

passed faster than the salt, *i.e.*, the ratio salt absorption to water absorption is less than unity.

This being so, we gather that a "normal" solution of sodic chloride is absorbed unchanged in percentage by a "physiological" action, but that with "weak" or "strong" solutions, the "physical" osmotic action must be superadded.

"In der that weisen schon manche der bisherigen Erfahrungen darauf hin, dass die Vorgänge bei der Resorption sich nicht einfach auf die eine oder die andere Weise deuten lassen, dass vielmehr physiologische und physikalische Factoren in einander greifen, mit je nach den Bedingungen wechselnder Energie, um die Resorption von Salzlösungen herzustellen" (Heidenhain).

Heidenhain, therefore, proceeds to study more minutely the absorption of sodic chloride solutions above and below "normal" strength, upon the assumption that in all cases a "physiological" absorption at "normal" strength occurs, but that the net result is affected in one direction or the other by the concomitant osmotic conditions.

With a solution of sodic chloride in the gut, of higher percentage than the blood (1 to 1·5 per cent. solutions were used), "physically" sodic chloride must pass to the blood, and water must pass into the gut as the total osmotic pressure of the solution in the gut exceeds that of the serum. Since, however, the actual experiments show that the water is absorbed from the gut under the circumstances, the "physiological" output of water exceeds the "physical" income. The absorption, then, of this "strong" solution of salt may be divided into a "physiological" moiety in which salt and water are passing out of the gut, and a "physical" moiety in which salt alone is concerned, for the "physical" water stream is swamped by the opposing stream of "physiological" origin.

Thus the salt absorption here is partly "physical" and partly "physiological," while the water absorption is entirely "physiological".

If the percentage of salt in the "strong" solution be raised to about 2 per cent., the increase in osmotic pressure

may be such that there is, as regards water, a balance between the "physiological" absorbing force and the osmotic pressure. Salt, however, will, under such conditions, continue to diffuse into the blood, and as the osmotic pressure in the tube of the gut is thereby lowered, at a later period, as the incoming osmotic stream of water wanes, it becomes more and more swamped by the opposing "physiological" current.

"So bewirkt die physikalische Diffusion für die physiologische Triebkraft Befreiung von den Fesseln, welche zu hohe endosmotische Spannung des Darminhaltes ihr anlegte, indem sie diese Spannung vermindert."

Turning now to the case of "weak" solutions of sodic chloride (·3 to ·5 per cent.), *i.e.*, solutions with a lower percentage than the blood, we see that "physically" water must pass to the blood with its higher osmotic pressure of salt, and salt should diffuse into the gut; but since here, again, there is a "physiological" output of salt and water, and since the actual experiments show that salt really disappears from the gut, the "physical" salt stream must be swamped by that of "physiological" origin.

In the case, then, of "weak" solutions as contrasted with "strong," the salt absorption is purely "physiological," the water absorption partly "physical" and partly "physiological".

There are then three distinct reasons for the hypothesis of a "physiological" action in the absorption of salt solutions :—

1. Fluids with the same total osmotic pressure as the blood are rapidly absorbed.

2. Water enters the blood from sodic chloride solutions in the gut, whose osmotic pressure exceeds that of the blood.

3. Salt enters the blood from sodic chloride solutions in the gut, whose partial pressure in sodic chloride is less than that in the blood.

The results of diminishing the physiological "triebkraft" will be different in the case of the absorption of the "strong" and the "weak" solutions.

With "strong" solutions, where the water absorption is entirely "physiological," while the salt absorption is partly "physiological" and partly "physical," the net result of interfering with the "physiological" action must be to reduce the absorption of water out of proportion to that of salt.

On the other hand, with "weak" solutions, where the salt absorption is purely "physiological," while the water absorption is partly "physiological" and partly "physical," the effect will be a diminution of salt absorption out of proportion to that of water absorption.

Heidenhain has found in sodic fluoride, added to ·04 to ·05 per cent. of the solutions, a means of poisoning the epithelium, and he finds that when added to the "strong" solutions of sodic chloride, undergoing absorption in a loop of dog's gut, the number representing the ratio of salt absorption to water absorption increases. As we would expect, the absolute salt absorption is found to be also diminished, as well as that of water, but the latter out of all proportion to the former.

The action is recovered from, but only slowly; thus in one case using one and the same loop of gut, it was found that after contrasting the absorption without and with the addition of sodic fluoride, a third experiment without the fluoride showed that it took the loop forty minutes to do the absorption work that before had been accomplished in twenty.

With "weak" solution of sodic chloride the reverse results are obtained; the salt absorption is diminished out of all proportion to the water absorption, so that the ratio salt absorption to water absorption falls, as it should according to the theory. In one experiment the water absorption was not halved, while the salt absorption fell to a seventh of that occurring before the use of the fluoride.

Sodic fluoride, then, is a reagent affecting the water absorption more when "strong" solutions of sodic chloride are undergoing absorption, the salt absorption more when the solutions are "weak," *i.e.*, it affects in each case that part of the total absorption which is to be regarded as of "physiological" rather than "physical" origin.

The result is not due to any raising of the total osmotic pressure of the solution in the gut by the addition of the small amounts of sodic fluoride. The result of raising the total osmotic pressure of a "weak" solution of sodic chloride in the gut by adding, say, sodic sulphate, is that the passage of water out of the gut is diminished, and though by deleterious action on the active-cells the output of sodic chloride might also be affected, the effect upon the water stream must be predominant. Experiment substantiates this, *i.e.*, an exactly opposite result to that got with the fluoride. Moreover, the action of sodic sulphate is only manifest when it is actually present in the gut, while, as seen above, that of fluoride lasts some time after its removal.

Finally, Heidenhain gives the case of two isotonic solutions of different salts placed in the gut. If water is absorbed at all from such solutions, it should, on the purely physical theory, be absorbed at the same rate in each case. But water is absorbed far more quickly from a sodic chloride solution than from an isotonic solution of magnesium sulphate; indeed, a sodic chloride solution whose osmotic pressure exceeds that of the blood loses water quicker than a magnesium sulphate solution whose osmotic pressure is below that of the blood, and the higher "osmotic equivalent" of the magnesium sulphate is not worth reckoning with in such conditions, for the total amount of the salt entering the blood is so small, that its "equivalent" of water entering the gut is but a minute fraction of the large amount of water actually absorbed.

We have referred to this last paper of Professor Heidenhain's at some length, since it represents the results of work that has been going on at Breslau for some years, and the opinion of one highly qualified to treat the subject. It must, however, here be noted that the interference with an artificially set up osmotic stream by the use of a physiological depressant, is not here demonstrated for the first time. The effect was demonstrated, now four years back, by Waymouth Reid, who, in the absorbing skin of the frog, showed not only that chloroform diminished a stream of

normal saline caused to pass osmotically from the outer to the inner surface, but that weak alcohol caused an augmentation.

Some idea of the value of the pressure exerted by the "physiological" action is given by Heidenhain, for if water is absorbed from a sodic chloride solution of higher osmotic pressure than the blood, the pressure against which the absorption occurs can be calculated by the difference between that of the solution and the serum.

In one case, a 1·46 per cent. solution of sodic chloride with $\Delta = \cdot 9$ placed in the gut lost water to the blood with $\Delta = \cdot 6$. The available pressure against the physiological stream which is overcome is represented by $\Delta = \cdot 3$, and this corresponds to a pressure of 2700 mm. of Hg. One can only understand the cells as being capable of withstanding such pressures, by thinking of the extremely minute capillary channels through which the fluids must flow.

The actual rapidity of absorption, though slow, seems to be far quicker in the gut than in an osmometer under somewhat similar conditions. As a maximum, about ·7 cb. mm. of water is absorbed by 1 sq. cm. of absorbing surface (with allowance for the extension of surface produced by the villi) per minute, so that a depth of 7 μ of fluid passes into the cells in a minute. Since a cell is about 35 μ in depth, it takes about five minutes to complete the journey from top to base, a result agreeing well with an experiment of Lehmann's, who detected potassic iodide in a mesenteric vein five minutes after placing the solution in the ileum. An osmometer closed with cow's bladder and filled with defibrinated blood, gives a transfer of only ·023 cb. mm. per sq. cm. per minute, while even with saturated sodic chloride solution in the osmometer, the rate was but ·55 cb. mm. of water per sq. cm. per minute.

As is to be expected, if a physiological act of absorption be admitted, differences of absorptive power should exist in different regions of the alimentary canal, though the circumstances for diffusion are much the same.

Tappeiner showed that in dogs, whilst in the duodenum and jejunum injected taurocholate and glycocholate of soda

were not absorbed, in the ileum both salts rapidly disappeared after their introduction. Again, that watery solution of strychnia was absorbed fast from the intestine but hardly at all from the stomach, though the addition of alcohol at once caused it to be taken up in the latter situation.

Edkins found the absorption of water greatest in the large intestine, hardly occurring at all in the stomach, and taking place at a less rate in ileum than in large gut.

Von Mering, in dogs with duodenal fistula, also found no absorption of water in the stomach (confirmed by Gley and Rondeau), though sugars, in accordance with the older work of Funke, von Becker, Meade Smith, and more recently Albertoni, were absorbed practically in ratio to their concentration.

Lannois and Lépine maintain that glucose is better absorbed in the upper jejunum than in the ileum of the dog, though this is doubted by Röhmann.

Again, Olschanetzky demonstrated the rapidity of absorption of watery solutions in the large gut, by detecting iodine in the saliva of a man five minutes after injection of potassium iodide into the rectum.

The subject of the absorption of watery solutions can hardly be left without a passing reference to the interesting chemical changes wrought by the intestinal cells upon some of the substances undergoing absorption.

Salvioli found that though peptone introduced into a loop of gut with artificial circulation was absorbed, yet it could not be recovered as such in the blood traversing the intestinal wall, and Hofmeister had previously shown that it disappeared somewhere in the mucous membrane. The peptone is probably "regenerated" to albumin, and since the theory of Hofmeister that this function is performed by leucocytes has been disproved by Heidenhain and Shore, we are probably safe in concluding with Neumeister that it is the columnar cells that effect this change during absorption.

Again, since dextrose is the blood sugar, and maltose the form in which sugar is presented to the intestinal epithelium, a change must be wrought during absorption. The

conversion was here originally considered to be brought about by the cells of the Peyers patches (Brown and Heron), but more recent work by Miss Tebb seems to show that the mucous membrane of the intestine is far more active in the process, though it must be admitted that many tissues effect the change.

The interesting syntheses of neutral fats from fatty acids will be referred to below.

It is not my intention in this article to follow the absorption of solutions any further, but it must be stated that the fact that such solutions reach the organism by the blood stream instead of the lacteals is, as Heidenhain has pointed out, easily accounted for by the fact that the capillaries of the villus lie in contact with the absorbing cells. If, however, a great excess of solution is present in the gut, as Ginsberg has pointed out for sugar, and Wertheimer for solutions of indigo carmine, a diffusion within the villus parenchyma may go on into the lacteal, so that the substances appear in the chyle.

If we now consider briefly the question of the absorption of fats, it will be evident that in spite of numerous researches, the actual process is far from being understood.

With regard to the question whether the fat passes through the bodies of the columnar epithelial cells, in the cement between them (Watney), or is transported by wandering cells (Schaefer and Zawarykin), the majority of observers maintain that the first-mentioned channel is the one normally used. Fat certainly appears between the columnar cells in microscopic preparations at times, but it is probably forced there from the villus parenchyma, during the contraction of the villus muscle that occurs when the tissue is plunged into the fixative, for fat is not so found between the cells of the non-muscular processes of the frog's gut (Heidenhain). Fat granules in leucocytes also undoubtedly occur, but Zawarykin's idea of the great prevalence of fatty leucocytes during digestion is, according to Heidenhain, an error of observation due to mistaking the granules of the oxyphil leucocytes for fat on account of their black reaction with osmic acid.

If it be decided that the fat is taken up by the columnar cells, we ask, in what form is it absorbed, and by what means do the cells accomplish their end?

There are two main theories: the one, that the fat is absorbed in corpuscular form as minute globules of an emulsion; the other, that it is in some way first got into solution, and the solution absorbed, the fat being subsequently again separated from the solution.

It has been advanced against the corpuscular theory, which appears to be the more prevalent of the two, that the epithelial cells refuse to take up minute particles offered to them.

Donders could get no evidence of "choroidal" pigment grains in the cells after feeding the animals on eyes, and Grünhagen could not succeed in making the cells lining the frog's gut take up particles of carmine. Tomasini has, however, lately maintained that starch granules, as such, can be taken up and recognised *in situ* by iodine. Then, again, in the microscopic preparations the fat particles are not seen in the basal band, or even immediately beneath it as a rule, but first appear at some distance within the protoplasm of the cell. The emulsion, too, of fat within the digesting gut is said by Cash to be not fine enough for the purpose, and Krehl, who fixed the whole gut with its contents in the case of *Triton* fed with cream, while he saw fine fat grains in the cells, only found conglomerate masses in the lumen of the gut. Certainly the fat does not attain the fine state of division which we know as the "molecular basis" of the chyle until it has actually passed the wall of the central lacteal, and unless we imagine a special "selective power" for the cylinder cells, it is difficult to explain why they should refuse a carmine particle but take a fat grain. Even the exsected gut of the frog, as Grünhagen has shown, will take up olive oil, especially when it is also supplied with the animal's bile, but refuses carmine particles.

The theory of the absorption of fat in solution was originated by Perewoznikoff, and shortly after developed by Will. The idea is that from the fatty acids set free by the action of the pancreatic ferment, soaps are formed with the

alkali of the bile and pancreatic juice, the soaps and glycerine absorbed, the fatty acid again set free in the cell (the alkali perhaps being used again), and recombined with the glycerine to form neutral fat.

Will fed frogs on pillules of pure palmitic acid, the melting point of which is, of course, far above the body temperature of the animal, and found distinct fat globules in the cells of the gut. He states that he controlled the results with observations on the appearances in the gut of starved frogs; a necessary precaution, since it has been shown by Grünhagen that the gut cells of starving winter frogs are loaded with fine fat grains, though the appearance is a very different one to that seen in a frog fed with fat. Ewald, too, kept exsected gut mucosa at body temperature in a solution containing soap and glycerine, and also maintains that he got evidence of fat grains in the cells.

Altmann has suggested a solution of the free fatty acids in the bile salts, a fact demonstrated to be possible by Strecker and Latschnikoff, and naturally maintains that his "granula" are the active agents in the regeneration that follows absorption.

Now, we know from the work of Immanuel Munk not only that considerable amounts of fatty acid can be detected in the gut of a dog at any time during the digestion of a meal of the neutral glyceride, but also that animals may be nourished upon fatty acids as well as on fats, provided an equivalent amount is supplied; and further, that the fatty acids given by the mouth appear in the chyle to a large extent, though not exclusively, in the form of neutral fats.

A few years back, a lucky case of chylous fistula in a patient enabled Munk to demonstrate upon man the truth of these assertions. In this case, practically the whole chyle flowed at intervals from the fistula. Olive oil by the mouth was collected as such at the fistula, mutton fat gave a chyle fat solid at the temperature of the room; but erucic acid from rape-seed oil gave the neutral fat erucin.

But the most elegant experiment was with spermaceti. This has a melting point of 53° c., and is a compound of palmitic acid and cetyl-alcohol; 14 per cent. of the weight

of the spermaceti eaten was collected at the fistula, but the material collected melted at 36° c., and was not spermaceti but ordinary tri-palmitin. Thus, the spermaceti was split, and the free fatty acid finding glycerine, from what source we know not, appeared as the neutral glyceride, the cetyl-alcohol not being detected.

Again, the fat of the chyle is known not to be capable of accounting for all the fat absorbed from the gut, so that even the supporters of the corpuscular theory must admit either that some soluble material is absorbed by the blood path, or that the deficit is metabolised *in situ*. Frank ligatured the thoracic duct, and still found that some portion of the fatty acids with which the animal was fed was being absorbed, but his blood analysis gave no help; further, he found the rapidity of output of fat at the thoracic duct on feeding with fatty acids was far behind the rapidity of absorption from the gut, and this difference was more marked than when the animal was fed with neutral fats.

Those in favour of the corpuscular theory may advance the undoubted fact that there is a distinct relation between the melting point of a fat and its economical absorption, a fat fluid at body temperature being far better utilised than one that is solid. Yet Munk and Arnschinck show clearly that a certain amount of fat of higher melting point than the body temperature is certainly absorbed. Arnschinck, for instance, found that from 9 to 14 per cent. of stearin melting at 60° c., and which is fairly hard even at 40° c., was absorbed. To what extent the bile is necessary is not very clear. V. Wistinghausen's theory of its action is hardly needed if we deny the presence of capillary tubules in the "basal band" of the cylinder cells. Dastre has certainly shown that if a dog is fed on a fatty emulsion such as milk, it can absorb fat without bile, and gain in weight; and Munk also had a dog with biliary fistula absorbing 100 grammes of lard a day, though with fats of higher melting point he admits the necessity of the presence of bile, for while 90 per cent. of mutton fat is absorbed by a normal dog, only 36 per cent. is taken up without bile.

In favour, perhaps, of the theory that fats must be split before absorbed, is the fact that Nencki found the absence of bile lessened the splitting power of the pancreatic juice, and Munk finds that neutral fats are not so well absorbed (by 6 per cent.) as free fatty acids by dogs with biliary fistula.

In fine, since the only histological observation of the actual corpuscular theory of absorption of fat by the cells is that of Thanhoffer, an observation which, we repeat, needs confirmation, we must at present reserve judgment as to whether the fats enter the cells in some form of solution, or are actually "swallowed" in fine particles. The droplets, once in the cells, are forwarded to their deeper ends, and then discharged into the fluid filling the meshes of the villus parenchyma. From here, probably, by the action of the pressure produced by the villus-muscle, they are forced in suspension through the wall of the lacteal, and this possibly causes a finer division, for the "fett-staub" of the chyle is composed of far finer particles than those seen within the sponge of the villus.

We can only understand the progress of the fat particles within the cells as being produced by some kind of motion analogous to "streaming movements" of protoplasm, but this has not been definitely observed, unless perhaps by Spina, in the cells lining the gut of the common house fly, and when we come face to face with the ultimate cause of that "physiological" absorptive action for fluids, spoken of above, we can only imagine something of the same kind.

Many have admitted active changes of shape of the cylinder cells, but Spina's observations form the only instance in which it has been definitely maintained that a regular periodic change of shape is visible during an act of absorption.

The gut cells of fly maggots, especially when fed upon frog's muscle stained with methyl violet, Spina maintains, can be seen to swell at their free ends, suck up the violet solution, and then contract and pass the solution towards the body cavity. He says that these cells refuse to take up the coloured solution from the body cavity and "secrete" it into

the gut; the cells only load from the gut, and discharge into the body cavity. He further maintains such movements are visible in the cells of the absorbing skin of the frog, when the edge of the web is observed in profile.

Heidenhain is doubtful of these observations, on account of the ease with which passive changes in the shape of the cells produced by contraction of the musculature of the gut may be mistaken for those possibly of active origin.

Whatever may be the final decision as to the mode of production of "physiological" secretory and absorptive pressure, it must be admitted that the time has now arrived when the latter must be as definitely accepted as has been the case with the former for more than forty years.

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E. WAYMOUTH REID.

COAL: ITS STRUCTURE AND FORMATION.

PART I.

"AMONG the humiliating proofs of our limited powers of inquiry, there are few which are more striking than that which is manifested by the inefficiency of our investigations relative to coal."[1] With these words, eighty-three years ago, Parkinson began one of his letters "on the opinions respecting the formation of coal". Since his day our knowledge of the Coal-Measures has gradually increased, but we cannot yet afford to regard his words as entirely inapplicable to our present position. In another letter, the same writer thus sums up his own views as to the manner of coal formation: "The opinion, which the strictest examination of every circumstance seems best to warrant the adoption of, appears to be, that coal is a product of the vegetable matter which has been buried at several distant periods, but chiefly in consequence of a universal deluge; and which, after having been reduced to a fluid state by the bituminous fermentation, has suffered a certain modification of that inflammability which bitumens in general possess, by the deposition of its carbon, and by an intimate and peculiar intermixture with various earthy and metallic salts".[2]

It would take us far beyond the limits of a single article to make any serious attempt to follow the gradual growth of our knowledge of coal, or the development of the theories which have, from time to time, been propounded as to its mode of formation. Some of the earlier theories of coal formation, such as we find in the works of Sternberg[3] and Link,[4] regard the seams of coal as the altered accumulation of masses of vegetable matter deposited as water-borne sediment. The latter writer, whose pages contain many

[1] Parkinson, vol. i., p. 233.
[2] *Ibid.*, p. 248.
[3] Sternberg, Fasc. i. and ii.
[4] Link, p. 43.

suggestive expressions of opinion, far in advance of the general thought of his day, refers to the occurrence of upright stems in the Coal-Measures, and realises the possibility that a vertical position may sometimes be the result of the action of water, and that it is not necessarily a proof of growth on the spot.[1] Within the last few years the elaborate memoirs by French geologists on the coalfields of Central and Northern France, have clearly demonstrated that in certain cases the true explanation of coal formation must be sought in the direction suggested by some writers in the early part of the century. The remarks by Link with reference to erect stems are especially interesting in view of the opinions recently expressed by Fayol[2] on the same subject.

We may approach the subject of coal from various points of view, and any one of these suggests diverse lines of inquiry which have not as yet been thoroughly exhausted.

We may turn our attention to the numerous questions of special geological interest which centre round the nature and mode of origin of the coal seams ; we may study the composition of coal in the laboratory with a view to solving the problems connected with the conversion of vegetable *débris* into a hard and compact rock ; we may, on the other hand, take up the investigation of the coal strata from the point of view of botanists, anxious to learn something from the fragmentary remains in the Palæozoic herbaria as to the lines of descent of existing plants ; and, finally, we may make more or less feeble attempts to picture to ourselves the actual geographical and climatal conditions which obtained during the building up of the great series of strata included in the Coal-Measures. The immediate purpose of this article is to draw special attention to some comparatively recent researches into the conditions of coal formation. The usual explanation of the manner of accumulation of coal seams, which we are accustomed to in English text-books, has

[1] Link, p. 44.

[2] The works of the various authors will be found in alphabetical order in the bibliography accompanying this article.

gradually come to be distrusted as inadequate to explain certain facts.

In 1842 Logan drew the attention of geologists to the constant occurrence in the South Wales coalfield, and in other districts, of a characteristic unstratified argillaceous rock underlying each bed of coal. It was shown by this observer that the underclay not only occupied this constant position, but that it was further characterised by containing numerous stigmarian remains. The idea of an old surface soil was gradually accepted as the most satisfactory interpretation of these facts. It was held that the beds of coal had been produced from a thick mass of vegetable *débris*, which had accumulated during centuries of forest growth on the underclay surface soils ; and in the stigmarias were recognised the forked roots of sigillarian stems which had largely contributed to the substance of the overlying coal. Bowman and others gave strong support to this view, and the former, in his theory of intermittent and irregular subsidence, found a convenient means of explaining certain peculiar features in the arrangement and relative positions of seams of coal.

During the last fifty years there have been numerous writers who have warmly advocated the theory that the coal-forming materials accumulated on the surface of forest-covered areas, and that, after subsidence had brought about a general submergence, the vegetable remains became sealed up under a covering of mud and sand.[1] Leaving out of account any differences in detail, the general concession of opinion has, until recently, been strongly in favour of this so-called growth-in-place theory of the formation of coal. It has come to be regarded as the orthodox standpoint from which to explain most conveniently the facts of Upper Carboniferous stratigraphy. We may briefly summarise some of the main arguments quoted in support of the growth-in-place views : (1) The almost constant occur-

[1] An interesting account of coal building on drift theory lines will be found in *Coal* (see bibliography). Hull devotes a chapter of his book on the British coalfields to this subject.

rence of the underclay and its stigmarian fossils under every bed of coal; (2) the remarkable absence of arenaceous or argillaceous impurities, and the uniformity of some coals in composition and thickness over a wide tract of country; (3) the not uncommon occurrence of upright stems of trees in strata associated with the beds of coal. Various authors have successively passed on these duly accredited arguments, without always pausing to think whether or not they form a fatal objection to some other mode of origin than the carbonisation *in situ* of a semi-decayed mass of forest *débris*. A glance at a series of chemical analyses[1] of anthracite, coal, lignite, peat and wood, shows a gradual increase in the percentage of carbon, and a corresponding decrease in other elements. From these and other classes of facts, it has been argued that we have in coal and anthracite the extreme terms of a fairly continuous series of vegetable deposits, which, speaking generally, are richer in carbon, according as they belong to older rocks, and have been longer exposed to slowly acting chemical changes. A connection between an increase in carbon percentage and the amount of earth movement to which the strata have been exposed, lends support to such opinions.

The unusual character of Carboniferous lignitic deposits in Central Russia, made up of paper-like laminæ of little altered corticular tissues,[2] has been attributed to the escape of the beds from the effects of earth movements, and from the influence of those potent factors, heat and pressure, which, in other cases, have accelerated and extended the chemical changes to a much more advanced stage in the process of carbonisation. Among Tertiary rocks we occasionally find carbonaceous deposits which would be placed in the category of ordinary coal, if they were not members of a much more recent geological system. Pressure and heat may have played important parts in the production of coal; but the series of changes involved in the alteration of plant tissues into compact coal, have been far

[1] Toula, p. 22, and in many other works on coal.

[2] Figures of these tissues are given by Zeiller in a paper published in the *Ann. Sci. Nat.* (Bot.), No. 6, vol. xiii., p. 213, 1882.

too complex to allow us to assign definite reasons for the present form of carbonaceous deposits. The element of time is constantly referred to as one of the guiding factors in the formation of lignitic deposits and true coals, but granting its importance in geological changes, it has, in all probability, been drawn upon too freely as a means of accounting for certain phenomena. The evidence of recent research seems to point very distinctly to a much more rapid formation of coal than has previously been supposed. It would seem that we have no good grounds for asserting that modern peat formations, or certain Mesozoic and Tertiary lignites, would ever assume the characters of true coals, however much time be allowed for future changes. There has been at certain times in the earth's history a concurrence of special conditions, which have rendered possible the formation of coal deposits on a large scale, and these conditions were especially characteristic of Upper Carboniferous times. We are certainly not justified in adopting Lesquereux' dictum[1] that peat bogs are nothing but beds of coal "not entirely ripe or burned out". The central idea of the growth-in-place theory may be summed up in a sentence from Geikie's text-book of geology: "Each coal seam represents the accumulated growth of a period which was limited either by exhaustion of soil, or by the rate of the intermittent subsidence that affected the whole area of coal-growth".[2]

Following the early views of Sternberg and others, several writers have in recent years advocated in some form or other the formation of coal strata by the drifting of vegetable *débris*, and its subsequent deposition on the floor of a lagoon or sea, with an accompanying series of arenaceous and argillaceous sediments.

Theories of this class which do not regard coal seams as old peat bogs, or as the remains of forests which grew on the underclay soil, are usually referred to collectively as the drift theory of coal formation.

For these opposing sets of views Gümbel[3] has instituted two new terms which have been adopted by a few writers;

[1] Lesquereux, p. 842. [2] Geikie, p. 808. [3] Gümbel, p. 201.

the growth-in-place method of formation he speaks of as the autochthonous origin of coal, and the building up of coal from drifted material is designated the allochthonous mode of origin.

Among the older views as to the nature and origin of coal, we occasionally meet with the suggestion that the greater part of the carbonaceous substance has been derived from bituminous eruptions. It has been generally agreed that coal is almost entirely made up of carbonised plant fragments, and the idea of any extraneous source of carbon has been allowed to drop. Recently, however, this old theory has been revived, and some new arguments set forward in its support. Before passing on to consider the allochthonous mode of formation, we must take note of this third theory which M. Rigaud has seriously discussed in a recent number of the *Revue Scientifique*.[1] This writer's main contention is that plants have played a subordinate *rôle* in the formation of coal, and are by no means mainly responsible for its production.

Coal usually exhibits traces of plant tissues embedded in a black substance, and this homogeneous matrix may be regarded either as a bituminous substance of volcanic origin or as a product of vegetable decomposition. Assuming a tropical climate for Coal-Measure times, Rigaud points out the unsuitable nature of tropical plants, and the still more unfavourable climatal conditions for the formation of anything of the nature of peat.

Neumayr and many other writers have drawn attention to the absence of peat in tropical countries, and have used this fact either as an argument against a tropical climate during the coal period, or as an obstacle to the growth-in-place method of accumulation.

Rigaud lays stress on the absence of certain elements in the ash of coals, which ought to be present, on the assumption that the carbon has been derived from plant tissues. If coal consists of altered vegetable *débris*, we ought to find a certain amount of alkalies and phosphoric acid in its ash ;

[1] I am indebted to Prof. Zeiller for calling my attention to this article.

the absence or very small quantities of these substances is, he contends, a serious argument against the generally accepted vegetable nature of coals. Had such substances ever been present, it is difficult to understand how they could well have been removed by the solvent action of water; the amount of maceration in running water necessary to eliminate these ash constituents, would probably involve the destruction of all traces of organic structures. It is always easy to fall back upon vaguely expressed chemical changes as a convenient means of explaining certain facts, but the point urged by Rigaud is one which should receive attention at the hands of those qualified to deal with this branch of the subject. The opaque black substance occasionally met with in the cavities of coal tissue cells, is regarded by this writer as so much bituminous material which has forced its way into the empty spaces. We have abundant evidence, he suggests, as to the eruption of hydrocarbons in past time in the bituminous shales, asphalte, and other similar substances. If such outpourings took place on a large scale on the floor of a lagoon or gulf into which water carried down vegetable and mineral sediments, the conditions would be favourable for the formation of beds of coal, and somewhat analogous to those which at present obtain in the pitch lake of Trinidad. The author of this latest theory of bituminous eruptions, claims for it that a critical examination of the facts and arguments should result in the verdict that it leads us a little nearer to the truth than the previously accepted explanations of coal formation.

Any attempt to explain the manner in which coal has been formed, must to a certain extent be founded on the facts of microscopic structure. The occurrence of distinctly marked impressions of plants on the surface of a piece of coal is fairly common, and in the absence of any definite markings imprinted on the surface, we can frequently detect a fibrous structure in the dull layers of mineral charcoal or mother of coal. In some places masses of coal are obviously made up of flattened pieces of *Sigillaria*, *Lepidodendron*, and other plants. Such instances are figured in Goeppert's famous dissertation on the structure and formation of coal.

In some French coals there are distinct signs that the rock is little more than a thoroughly carbonised mass of strap-shaped *Cordaites* leaves. A careful naked-eye inspection of some coals reveals the existence of small compressed circular or elliptical bodies which, on isolation and microscopical examination, are found to show a very striking resemblance to the macrospores of some recent pteridophytes. The marked similarity of the fossil and recent forms has been demonstrated by Williamson and other writers ; it is very clearly shown in some figures given by Kidston and Bennie in a paper dealing with Carboniferous macrospores.

The microscopic investigation of coal is necessarily attended with some difficulty owing to the opaque nature of the material. Some observers have adopted a method of examination by means of specially prepared semi-transparent sections ; others prefer to treat pieces of coal with potassium chlorate, nitric acid, and other reagents, in order to isolate the plant tissues. Suggestions as to manipulation and the preparation of sections will be found in the contributions of Gümbel, Dawson and others.

The great pioneer work of Witham of Lartington on *The Internal Structure of Fossil Vegetables*, includes a short description and a few figures of microscopic preparations of cannel and other coals. Shortly after the publication of Witham's results, Hutton recorded the occurrence of some light wine-yellow coloured material in the cavities of plant cells, as seen in thin sections of certain kinds of coal; this was regarded as apparently a bituminous substance, which he found to be readily expelled on heating. These observations of Hutton are of special interest in connection with some startling results recently published by MM. Renault and Bertrand on the boghead coals of Central France, Scotland, and Australia. In 1838 Link gave some account of the microscopic structure of peat, lignite and coal ; he figures various fragments of plant tissues, and small resinous orange-coloured bodies, the nature of which he leaves undecided. He regards coal as the peat of a former geological age. In 1857 Bennett made a detailed examination of the structure

of the Scotch boghead or torbanite, a substance rendered famous by the legal proceedings in the case of Gillespie *v.* Russel,[1] and by the scientific evidence of experts in favour of and against its close connection with coal. Bennett figures some sections of ordinary coal showing crushed resinous spores, also sections of the boghead containing numerous transparent bodies of irregular outline, embedded in a black matrix. The bituminous boghead is considered to be essentially distinct from coal in the absence of obvious plant structures. Bertrand and Renault,[2] who have lately examined the Scotch boghead, give an entirely different interpretation to the yellow bituminous bodies of Bennett and others; they look upon them as the well-preserved remains of fresh-water algæ.

Balfour dissents from Bennett's views as to the nature of boghead, and prefers to class it with true coals; his paper on this subject contains a figure of an interesting example of "spore" coal.

Without attempting to give any complete account of the earlier contributions to our knowledge of coal structure, such as those by Quekett, Phillips, and others, a brief reference may be made to Huxley's description of what he regarded as sporangia and spores in the well-known Better bed coal of Bradford. Williamson[3] pointed out the fact that the so-called sporangia were macrospores, and the smaller bodies microspores; this palæobotanist gave a short preliminary account at the York meeting of the British Association in 1881 of his researches on the structure and physical composition of coal. For several years the task, which Professor Williamson set himself, of making a "systematic series of microscopical observations on the coals of the entire world," has been gradually proceeded with; and the publication of these researches should put us in possession of many important data, from which we may expect to obtain further light on the question of coal formation.

[1] An account of the evidence given at this trial by scientific experts will be found in Quekett's paper.

[2] See references given in Bertrand's paper, also "SCIENCE PROGRESS," vol. i., p. 60.

[3] Williamson (2).

Dawson adopted the method of chemical treatment in his investigations on the minute structure of coal, and has been able to identify numerous fragments of scalariform and pitted vessels, and other tissue elements; he was led to the conclusion that the existence of spore-bearing beds is an exceptional rather than a common occurrence. In speaking of the nature and origin of coal, Dawson sums up the question as follows: "In short, a single trunk of *Sigillaria* in an erect forest presents an epitome of a coal seam. Its roots represent the stigmaria underclay; its bark the compact coal; its woody axis the mineral charcoal; its fallen leaves, with remains of herbaceous plants growing in its shade, mixed with a little earthy matter, the layers of coarse coal."[1]

Some further information as to the occurrence of spores in coal is contained in a contribution by E. T. Newton, in which special attention is directed to the numerous spores in the Australian white coal or Tasmanite; the name *Tasmanite punctatus* was suggested for these spores, but it has not been generally adopted, and, indeed, such a designation does not seem particularly appropriate.

We are indebted to another English geologist, Wethered, for additional facts as to the various spore coals; he has figured and described several forms of macrospores and microspores in the Better bed and other seams. He points out the abundance of spores in the dull layers of certain kinds of coal, and suggests the rather unfortunate term, hydrocarbon, for the structureless substance which chiefly constitutes the bright patches in coal seams.

Brief reference must be made to the work of Reinsch, whose patient investigations resulted in the discovery of numerous structures in coal which he was at a loss how to dispose of in any existing classification of plants. He finally decided to consider them as plants of a specially primitive type; some he compared to Myxomycetes, and others were provided with special polysyllabic designations, and consigned to classes or groups instituted for their reception. In some of these structures he recognised a resemblance to sphærocrystals, but, unfortunately, the temptation

[1] Dawson (1), p. 638.

to regard them as unknown forms of plant life proved too strong, and they were all laboriously figured and defined as new types of protophytic genera. The majority of Reinsch's genera must be regarded as inorganic structures, whilst others are clearly founded on plant tissues or spores. In addition to various fragments of vascular tissue he recognised several spores, and to these applied the generic term, *Triletes*, which some authors have found convenient. Following these investigations we have a paper by Fischer and Rüst, in which Reinsch's conclusions are called in question, and the various strange "plants" are regarded as examples of the numerous forms assumed by resinous and other substances which enter into the composition of coal. The same observers mention the occurrence of several definite crystalline substances in certain forms of coal, *e.g.*, Fichtelite, Hartite, etc. A detailed account of such bodies in coal rocks, and of the various forms of coal, lignite, etc., has lately appeared in the third volume of Zirkel's new edition of his *Lehrbuch der Petrographie*. In 1883 Gümbel published a valuable account of the minute structure of coal, lignite, and peat; his method of examination is fully described, and the results obtained bear out the advantage of a chemical treatment in certain cases. He points out that Reinsch included in his protophytic genera such structures as dendritic crystals of sulphur, and various other mineral substances. The same author treats of the method of conversion of plant tissues into coal, and disputes the accuracy of the common statement that anthracite is simply coal which has been subjected to greater pressure. The occurrence of anthracite not merely in the deeper layers of a coal series, but between or above ordinary beds of coal, suggests some other factor than increased pressure and metamorphism. In discussing the question of coal formation, Gümbel recognises that there must have been different methods by which the same results were obtained; but, on the whole, he is disposed to agree that there are good grounds for the comparison of Palæozoic coal seams and modern peat formations.

The important memoir on coal by Grand' Eury[1] contains

[1] Grand' Eury (1).

a considerable amount of information as regards the microscopic structure; he discusses the source and manner of formation of the mineral charcoal (Fusain or Faserkohle), and considers that it has been formed by the breaking up of the woody interiors of many of the flattened hollow stems which are so common in coal and the associated shales. The separation of bark and wood, and the gradual disintegration of the latter, are phenomena which may be observed in present-day forest trees.[1] In amorphous coal, Grand' Eury considers we have the result of a precipitation of ulmic substances, with spores and other parts of plants on the floor of a lake or sea. In their recent monograph on the fossil plants of the Commentry coal field, Renault and Zeiller devote some pages to the microscopical examination of coal. The cannel coal of Commentry is compared with that from Lancashire; both consist largely of an amorphous substance, with occasional spores and tissue fragments.

Anthracite is more difficult to examine, and shows less organic structure. The boghead is rich in inorganic matter, but also contains numerous small lenticular bodies exhibiting fine radiating lines, extending from the centre to the periphery, where they lose themselves in a mass of fine granulations.[2] In ordinary coal portions of carbonised stems of *Calamites*, *Psaronius* and other plants are occasionally met with.

Allusion has already been made to the Scotch torbanite or boghead; this, with similar carbonaceous beds from Autun and Australia, has recently been the subject of an article by Bertrand, who has previously published some researches on the same subject in collaboration with Renault. The bogheads are described as coals which yield, on distillation, a large quantity of very bright gas. Examined microscopically they reveal the existence of numerous golden yellow balls possessing a radiate structure; occasionally there may be as many as two hundred and fifty thousand to one million of these minute spherules in one cubic centimetre. A close examination of these structures leads Bertrand and Renault to regard them as the thalloid bodies

[1] Solms-Laubach (1), p. 24. [2] Renault and Zeiller, Pl. lxxiv., fig. 9.

of a gelatinous alga, to which the name *Pila bibractensis* has been assigned. The thallus is divided up into a number of small thick-walled cells, and in some of the cell cavities—thanks to their wonderful preservation in silica—protoplasm and nuclei have been recognised. We may be permitted to express grave doubts as to the possibility of such well-nigh incredible statements. In addition to the numberless examples of *Pila bibractensis*, pollen grains are not uncommon. The algal thallus and pollen grains are embedded in a brown ground mass which constitutes a kind of amorphous precipitate charged with vegetable fragments. The conclusion arrived at is that the Autun boghead is the product of an immense accumulation of a single species of gelatinous alga, with grains of pollen and other plant structures, in a matrix of ulmic substances. The brown colour suggests the coffee-coloured waters of some tropical rivers, and the algæ may be regarded as analogous to the *fleurs d'eau* of fresh-water lakes. In the calm, brown waters of a Permo-Carboniferous lake, ulmic materials were precipitated by the action of carbonated waters ; and at certain seasons of the year the surface of the water was covered with a mass of microscopic algæ, and these, with showers of pollen from neighbouring forests, accumulated as a pulpy mass of ulmic products on the flora of the lake, and so gave rise to a deposit of boghead. A similar structure is recognised by these authors in an Australian boghead, and in the torbanite of Scotland. In the Australian boghead *Reinschia australis*, another gelatinous alga, has played the most important *rôle* in building up the carbonaceous material ; and in the Scotch rock another species of *Pila* is the characteristic constituent. In addition to the algal species, an aquatic Myxomycete is recorded from the Autun boghead, described under the name of *Bretonia Haidingeri ;* the same genus has also been found in the Scotch beds.

It should be noted that the Autun bogheads may occur mixed with ordinary coal, and that coal is sometimes found in the form of lenticular patches in a bed of boghead. The results arrived at by Bertrand and Renault compel us to adopt a somewhat sceptical attitude in attempting to form an

opinion as to the facts they record. We are accustomed to find in the petrified remains of Permo-Carboniferous plants every detail of wall sculpture and cell outline faithfully preserved, but to have millions of examples of a gelatinous alga with cells, and even cell contents, clearly defined, is a revelation which borders on the miraculous.

Have we sufficiently good evidence before us that the boghead structures are really gelatinous algæ? Must we look upon the bright, yellow bodies in these rocks as the result of the phenomenon which we are familiar with on a smaller scale in the breaking of the meres? In the first place it may fairly be asked: Do the descriptions and figures of *Pila bibractensis* show a marked resemblance to any known form of alga? Bertrand gives numerous drawings of this species, showing what he believes to be different stages in the development of the thallus. The general appearance of the structure does not suggest any distinct resemblance to any type of recent plant, and it is difficult to understand with what form of alga these Palæozoic specimens may be best compared. The existence of protoplasmic and nuclear substances in a fossilised condition, as described by Bertrand and Renault in the case of the boghead structures, and by Lignier[1] in Bennettites, can hardly be credited; it is true we frequently find a black substance in the secretory canals of fossil plants, but the preservation of carbonised resins or gums is much more intelligible than the mineralised remains of protoplasmic material. It is hoped than an examination of boghead sections may lead to a more definite expression of opinion on the nature of these cellular structures, but at present it is very difficult to accept the published results as to these minute yellow-coloured bodies. Without venturing to speak at all dogmatically, the more probable conclusion seems to be that we have here to deal with curious inorganic structures which closely simulate the cellular structure of plants.

A. C. SEWARD.

(*To be continued.*)

[1] For notice of Lignier's excellent paper see *Nature*, p. 594, Oct., 1894.

THE COAGULATION OF THE BLOOD.

THE causes that lead to the clotting of blood form a subject which seems to possess a peculiar attractiveness to the investigator of physiological problems. Like the subject of muscular contraction it produces every few years a fresh crop of theories seeking to explain it. But in both cases, the new facts that are discovered often throw fresh difficulties in the way of, instead of shedding new light upon, the vexed question at issue.

It is, however, useful to pause every now and again, and take stock of the scientific position in matters of this kind. We must start by frankly acknowledging that a final and conclusive theory has yet to be discovered, but a historical retrospect is by no means uninteresting and is often useful. I therefore propose in the following article to sketch briefly the story of blood coagulation in the hands of scientific investigators, and to compare our present knowledge with that of our predecessors. I think it will be acknowledged that we have made some progress of late years, but the progress has consisted rather in discovering our ignorance than in removing it.

When the microscope first came into use, it was recognised that the blood is not a homogeneous red fluid, but consists of a nearly colourless fluid, the plasma, in which are suspended a number of particles which are called the blood corpuscles. Of the two main classes of corpuscles, the red ones are by far the more numerous, and give the red tint to the blood as a whole. The white corpuscles are small and typical animal cells, masses of living material (protoplasm), containing a nucleus.

In those early days, the clot which occurs in blood after it is shed was supposed to consist merely of a mass of adherent corpuscles. Some held that they stuck together because the blood was no longer in active movement; others again thought the cooling of the blood after its removal from the body caused the corpuscles to form a coherent jelly, much in the same way that soup sets when

it is cold. We now know that agitation hastens and does not hinder coagulation, and that cooling hinders and does not hasten the process. We have, moreover, learnt that the clot contains something else in addition to the corpuscles; this something does not exist as such in the living blood, and it is of the nature of an insoluble proteid or albuminous matter. It is called fibrin, and, as its name implies, it consists of fibres or strings which bind the corpuscles together. The essential fact in coagulation is the formation of fibrin, and the causes of coagulation narrow themselves down to the causes of fibrin-formation or precipitation.

It was not until the close of the eighteenth century that an idea of a coagulable substance in addition to the corpuscles was mooted; the existence of fibrin was fully recognised by Hewson (1772), and taught by Fordyce and the Hunters.

Hewson obtained specimens of blood which coagulated with great slowness; when these were allowed to stand, the corpuscles settled towards the bottom of the containing vessel, leaving a clear layer of plasma at the top. This he skimmed from the surface, and found that after waiting a short time the strands of fibrin were slowly deposited till the whole fluid had set into what looked like a jelly, so close were the meshes of the network. Hewson further discovered the fact that cold, contact with living blood-vessels, and admixture with certain neutral salts are agencies which hinder or prevent coagulation by delaying or preventing the deposition of fibrin filaments. In connection with the influence of the living vessels on the process, the subject was at a later stage taken up by Lister, Fredericq and Brücke, who worked out many of the details in connection with this inhibitory influence.

Andrew Buchanan of Glasgow appears to be the next who made noteworthy investigations on this subject. He experimented with fluid obtained from the pericardial sac and from the tunica vaginalis in the dropsical condition of that serous membrane called hydrocele. These liquids resemble blood plasma very closely, but they do not coagulate spontaneously; Buchanan found that the addition

of small shreds of "washed blood-clot" caused the formation of fibrin in them. This power was exhibited to a still greater extent by the "buffy coat" of a clot; he therefore concluded that the power resided in the white blood corpuscles which are so abundant in the buffy coat, and with almost prophetic instinct, though he did not employ the term ferment, he compared this action to the action of rennet in curdling milk.

British investigators having cleared the way, we find the next important names among the workers on the Continent. First came Denis, who saturated blood plasma with sodium chloride, and thus obtained a proteid precipitate. This precipitate was collected, re-dissolved in a little water, and allowed to stand. The aqueous solution remained liquid for a short time, but soon a clot of fibrin made its appearance. Denis had thus obtained from the plasma the soluble precursor of the insoluble fibrin, and he named it plasmine. Denis' plasmine was soon shown to be a mixture of at least three substances, and to them the term fibrin-factors was, until quite recently, universally applied.

Alexander Schmidt, Professor at Dorpat, recognised these three materials and named them fibrinogen, fibrinoplastin, and fibrin-ferment. He considered that the two first-named substances, which belong to the globulin class of proteids, united together and formed fibrin, and the agency which caused their union was the fibrin-ferment.

Olaf Hammarsten, of Upsala, ascertained the characters of these substances with greater exactness, but his most important contribution to science was the discovery that the fibrinoplastin (or, as it is now called, paraglobulin or serum-globulin) was not essential. A solution of fibrinogen *plus* ferment will cause the formation of fibrin : the paraglobulin always remains, if present, in solution, though its presence hastens coagulation ; this faculty it was found to share in common with casein from milk, and even the inorganic salt calcium chloride.

The question of the causation of blood clotting had now narrowed itself still more. It was fibrin-ferment which caused fibrin formation ; the cause of the formation of

fibrin-ferment in shed, that is, in dead or dying, blood had next to be discovered. Here Schmidt and Hammarsten were agreed, and their lead has been followed by the greater number of subsequent investigators, that it was the disintegration of the colourless corpuscles which led to the shedding out of this new material.

The whole theory so propounded may be put briefly as follows: When the blood is within the blood-vessels, one of the constituents of the plasma, a proteid of the globulin class called fibrinogen, exists in a soluble form. When the blood is shed, the fibrinogen molecule is split up, the comparatively insoluble substance, fibrin, being the principal product of its disintegration. This change is brought about by a special unorganised ferment called the fibrin-ferment, which does not exist in healthy blood contained in healthy blood-vessels, but is one of the products of the disintegration of the white corpuscles that occurs when the blood leaves the blood-vessels or comes into contact with foreign matter.

Now this was a very good working theory; it possesses the merit of comparative simplicity, and is in accordance with the experimental evidence which was at the disposal of Schmidt and of Hammarsten. It is the theory which is given as gospel in most of the leading text-books on Physiology. But workers all round are beginning to doubt if it is true, or, at least, if it is the whole truth. There can be no doubt that fibrin-ferment prepared by Schmidt's method does cause coagulation in certain forms of plasma, obtained from the blood by preventing it from coagulating by such means as admixture with neutral salt. The weak point in the theory has always been recognised to be the fact that injection of fibrin-ferment into the circulation of a living animal does not cause intravascular clotting. Hence it was necessary to tack on to the theory the postscript that the living vessels possess in some way a power either to counteract the action of fibrin-ferment, or to destroy it.

Looked at from another point of view, too, the theory, after all, only shifts the matter a little farther back, for we have now to ask the cause of the disintegration of the

white corpuscles. This was explained by saying that the white corpuscles are exceedingly sensitive to change; while they are swimming happily round their circle from arteries to veins and from veins back to arteries again they are in their element, and so they do not break up, or only so slowly that the vascular wall is able to cope with the products of their death; but when they get out of their normal habitat, they break down, not in ones or twos, but in battalions, and the innocent fibrinogen falls a victim, and starts a new phase of existence as fibrin. This explanation is, of course, no explanation; it is hardly an apology for one; if we were able to say why the white corpuscles die we should be very near to explaining the mystery of the difference between life and death.

About the time, now nearly twenty years ago, when scepticism of this sort was simmering, some folks began to inquire whether it really is the fact that the white corpuscles break up when the blood dies; and to the late Dr. Wooldridge belongs the credit of pointing out that this fundamental point in the ferment theory rests on very flimsy evidence. Soon one of Schmidt's pupils, Rauschenbach by name, had to admit that there were two classes of white corpuscles, one class consisting of those which did, the other of those that did not disintegrate. Prof. Haycraft, too, was never able to see actual disintegration, though he described certain differences of appearance between the living and the dead leucocyte (white corpuscle). Observations made much more recently in my own laboratory have led me to follow in the wake of these observers, and I feel that the word disintegration must not be taken in the too literal sense in which it was used by Schmidt; it may be that the dying corpuscle sheds out material, though it does not break up into fragments.

Wooldridge thus started a new era in the history of the subject; he, perhaps somewhat hastily, propounded a new theory, part of which will perhaps stand. But whether it does or not matters but little, for, by breaking new ground, and inspiring healthy scepticism, he has succeeded by his own work, and by that of his followers, in introducing new

ideas, new methods of investigation, and new knowledge in consequence.

His work fell into three chief lines :—

1. He attributed to the corpuscular elements of the blood a secondary *rôle* in the causation of clotting. He considered that the *débris* of corpuscles described by Schmidt is analogous to, if not identical with, a precipitate he obtained by cooling peptone plasma.[1]

2. He attributed to a compound rich in phosphorus a very important part in producing fibrin formation ; he thought this compound was lecithin ; but all recent work tends to show that it is nuclein rather than lecithin, that is the phosphorised compound he was dealing with.

3. He discovered a material which, when injected into the circulation of a living animal, does produce intravascular coagulation ; and this material is not fibrin-ferment. This third branch of Wooldridge's work is the one which has borne most fruit.

When these views were first promulgated they met with a good deal of incredulity, and it appeared absolutely impossible to reconcile them with those of the older school of Schmidt and Hammarsten. Pekelharing was the first to attempt to reconcile the conflicting theories ; and he fancied he had discovered the connecting link in the relationship of calcium salts to the coagulation process.

Many years ago, Brücke demonstrated that the ash of fibrin always contains calcium. Later (1875), Hammarsten, as we have already noted, pointed out that calcium chloride can take the place of serum-globulin in aiding the action of fibrin-ferment. In 1887, Green found that the coagulation of various forms of plasma is much accelerated by small quantities of calcium sulphate ; and then Drs. Ringer and Sainsbury showed that the same property is possessed by other calcium salts, and to a less extent by the salts of strontium and barium. Freund, who had made somewhat

[1] Peptone plasma is plasma obtained from blood by injecting into the circulation of the animal before death a solution of commercial peptone. The active ingredients in the so-called peptone appear, however, to be albumoses.

similar observations, formulated a new and eminently simple theory of blood coagulation, which was almost immediately annihilated.

The next to enter the field were Arthus and Pagès, who made the brilliant discovery that the blood may be kept liquid by decalcification. This may be readily brought about by mixing the blood immediately it is shed with a 0·2 per cent. solution of potassium oxalate. They consider that fibrin is a calcium compound of fibrinogen, and their experiments further led them to believe that the ferment as well as the calcium salt is necessary for the transformation of fibrinogen into fibrin.

Green, in the work already alluded to, took up the question whether the fibrin-ferment exists in the blood as a zymogen (mother of ferment) which is changed into the ferment by the calcium salt. His conclusions, however, were negative. This point was taken up by Pekelharing, who prepared from various forms of extravascular plasma a substance with the solubilities of a globulin which possesses no fibrinoplastic properties, but which, by treatment with a calcium salt, assumes the fibrinoplastic activities of fibrin-ferment.

He therefore regards it as the mother substance of the ferment, and as identical with a substance I myself had previously described as cell-globulin. Pekelharing considers with Arthus and Pagès that fibrin is a calcium compound of fibrinogen, and that the main action of the ferment is the handing over of the calcium to the fibrinogen. Oxalates hinder coagulation because they precipitate the necessary calcium salts; and there is very good reason for believing that peptone acts in a similar way for a similar reason, namely, its affinity for calcium salts.

He then proceeded to examine the "tissue fibrinogens," the substances which Wooldridge discovered to be capable of producing intravascular coagulation. Like other observers he found their chief constituent to be nucleo-albumin; and in a later research he discovered that the globulin just alluded to, my cell-globulin, was also a substance of the same nature, yielding an insoluble residue of

nuclein on gastric digestion. In other words, the substances which have at various times received the names of fibrinoplastic substance, fibrin-ferment, cell-globulin, fibrinogen A., tissue fibrinogen, etc., etc., are all varieties of one substance which is nucleo-albumin, and further, that it is in all cases a nucleo-albumin which, in co-operation with calcium compounds, brings about coagulation in the blood.

In order to make this review complete, it is now necessary to allude to the recently-published researches of three more observers or sets of observers.

Wright has devoted himself to what may be an important side issue, namely, the influence of the amount or the tension of carbonic anhydride in the coagulating blood. It is certainly a fact that intravascular coagulation is generally most intense in the veins, where the pressure of this gas is greater than in the arteries.

Lilienfeld and Kossel, in Berlin, have also turned their attention to nucleo-albumins, and have dubbed them nucleohistons, on account of a supposed resemblance between histon, the proteid moiety of the material, and peptone.

And lastly, in conjunction with Dr. Brodie, I have recently published in the *Journal of Physiology*[1] an account of some experiments in the same direction, which we have been carrying on at King's College for the last three years; and I propose to conclude this paper by giving a brief summary of our results.

1. Nucleo-albumins may be prepared from most of the cellular organs of the body (muscle is an important exception) either by Wooldridge's acetic acid method, or by a new method in which alternate treatment of the tissue with sodium chloride and water constitutes the main feature, but for full details of which the original paper must be consulted.

2. The material obtained by both methods from the same organ is the same in (1) general reactions which closely resemble those of a globulin, (2) percentage of phosphorus, and (3) physiological action, *i.e.*, the production of intravascular coagulation; death is due to cessation of

[1] Vol. xvii., p. 135. References to other authors will be found in this paper.

respiration primarily caused probably in the respiratory centre. The nucleo-albumins, obtainable from various organs, differ in some minor points.

3. Protagon, the most abundant impurity in these preparations, and the closely-related substance lecithin, are not responsible for the clotting; neither is the dilute sodium carbonate which was used as a solvent.

4. The nucleo-albumins do not accelerate the coagulation of extravascular (dilute salted) plasma, and so contrast very forcibly with fibrin-ferment.

5. A few experiments with "peptone" and "leech extract" confirm the hypothesis of Pekelharing, that these substances hinder coagulation on account of their affinity for calcium.

6. Our experiments lend no support to the theory of Wright and Lilienfeld, that the negative phase of coagulation (*i.e.*, the production of non-coagulable instead of coagulated blood) sometimes observed is produced by the splitting off of a peptone-like substance from the nucleo-albumin. There is no evidence of peptone or albumose in the blood or urine of the animal under experiment, and the properties of the albuminous moiety of nucleo-albumin are entirely unlike those of peptone.

7. Failure to produce intravascular clotting is partly explained by idiosyncrasies of the animals used, and partly by certain factors in the preparation of the nucleo-albumin; these are discussed with full details and tables of analyses.

8. There appears to be some evidence that the nucleo-albumins active in producing clotting are preceded in the cells themselves by similar substances which differ from them in not possessing this remarkable physiological activity.

9. Solutions of nucleo-albumins destroy the blood corpuscles. This, however, is chiefly due to their alkalinity, and will not explain the thrombosis (intravascular clotting) produced. Distilled water, for instance, is a powerful solvent of blood corpuscles, but never produces intravascular coagulation. Löwit has recently given a list of agents that produce leucolysis (destruction of leucocytes) and states that *plus* calcium chloride they always cause thrombosis. We have repeated these experiments carefully, but were en-

tirely unable to corroborate Löwit's statement. Schmidt's view of the preponderating influence of leucolysis on blood-clotting is therefore not borne out.

I should like to add in conclusion that, although I regard Pekelharing's attempt to harmonise rival theories as extremely ingenious, it appears to me to break down in two points. The first of these is his idea that nucleo-albumin is the mother substance of fibrin-ferment, and convertible into fibrin-ferment by the action of a calcium salt. His experiments do not bear this out thoroughly, for the amount of calcium salt is presumably the same in extravascular as in intravascular blood. Yet nucleo-albumin causes coagulation in one, but not in the other.

The second point is his inclusion of fibrin-ferment among the nucleo-albumins ; there is no analytical evidence of this.

It is quite possible that nucleo-albumin and fibrin-ferment are related substances, perhaps related very closely. It is also quite possible that they are absolutely different substances. There certainly are differences : let me enumerate some.

a. Fibrin-ferment is not readily coagulated by alcohol ; nucleo-albumin is.

b. Fibrin-ferment cannot be obtained by Schmidt's method from nucleo-albumin.

c. Fibrin-ferment causes coagulation in extravascular (salted) plasma ; nucleo-albumin does not.

d. Nucleo-albumin causes coagulation in intravascular blood ; fibrin-ferment does not.

These last two differences form one of the greatest difficulties in properly understanding the method of fibrin-formation, or at least of reducing it to one common law.

Why, after all, is there a need for a common law ? May not the two substances in question be quite distinct from each other, each being capable of producing fibrin under suitable conditions ?

It is by no means inappropriate to close a paper on such a subject as blood coagulation with a number of unanswered questions.

W. D. HALLIBURTON.

INSULAR FLORAS.

PART III.

IN this part I propose passing in rapid review the most important botanical literature of the last decade relating to the arctic islands, more particularly those of the eastern hemisphere, and the islands of the Atlantic Ocean. In succeeding articles I intend taking the West Indies and the African islands of the Indian Ocean, followed by a brief examination and discussion of the facts and theories bearing on insular floras generally, especially in relation to the new lights of recent discovery and investigation. For this purpose I am desirous of adding to the bibliography any publications of moment that I may have omitted, and I should be thankful to my readers for any information they can give me on this point. Botanical literature has increased so rapidly during the last ten years, and is so widely scattered, that important contributions may easily be overlooked, even when one has the advantage of the most extensive libraries. Therefore my request.

The bibliography alone of the botany of the islands of the arctic and sub-arctic seas would fill several pages, even if we only go back as far as the last British Polar Expedition. I will therefore confine myself to a few of the principal publications in which students will find references to the less important ones. We are chiefly indebted to Scandinavian botanists for the more complete investigation of the arctic islands of the eastern hemisphere, and for some exceedingly interesting discussions on the history of the arctic flora generally. It is true that much of this literature deals with the origin of the flora of Greenland, which hardly comes within the scope of the present paper. The "Vega" and "Dymphna" Expeditions were especially fruitful. Among the islands to the extreme east of Asia recently visited are several of the Aleutian chain, on the Asiatic side of the strait, including Behring Island and two or three others, known collectively as the Komandarski or Com-

mander group (1); the name being very variously spelt. This is little more than a list of 132 species of vascular plants collected in Behring and Copper Islands, but it is supplemented (2) by a brief account of the plants collected in Behring Island by the "Vega" Expedition. Although in no higher latitude than 55°, or the same parallel as Newcastle, there is no arboreous vegetation, and the shrubs are few and small or stunted, being overtopped by tall herbaceous plants, characteristic of the grassy plains (3) of Kamtschatka. There is, perhaps, no cold country in the world where herbaceous plants attain such an astonishing size as in Kamtschatka, and although the insular plants do not attain equally large dimensions, Kjellman states that the shrubby vegetation is concealed by herbs commonly as tall as a man. Conspicuous among these are: *Cacalia auriculata, Senecio palmatus, Cirsiumkamtschaticum* (*Compositæ*), *Conioselinum kamtschaticum*, and *Heracleum lanatum* (Umbelliferæ). Species of many other genera grow to an unusual size, for example: *Pedicularis, Polemonium, Sieversia*, and *Aconitum*. Woody plants are represented by *Pyrus sambucifolia, Rhododendron chrysanthum* and *Salix arctica*. The plants named are characteristic of the slopes towards the sea; but the interior plateau has a heath-like vegetation consisting of *Bryanthus Gmelini, Cassiope lycopodioides, Arctostaphylos alpina*, and similar plants. Characteristic arctic plants are wanting, and Kjellman designates the majority of the species as arctic-tertiary, which probably formerly had a much wider distribution.

St. Lawrence, in the mouth of the straits, is another of the islands botanised (4) by the "Vega" Expedition; and the flora of the Western Esquimaux-land (5) is interesting for comparisons. A fragment of the tertiary flora of the islands of New Siberia (6) leads the author to the conclusion that they have been separated from the continent in comparatively recent times. The coniferous element predominates, and associated with it is a brittle kind of amber, such as occurs on the mainland. *Sequoia, Dammara, Taxodium, Pinus*, and *Cupressinoxylon* are the genera recognised.

Novaia Zemlia has been by this time so exhaustively explored that little can remain undiscovered; and the same may be said of Spitzbergen. Th. Holm has brought together and discussed in some detail (7) the composition and physiological characters of the vegetation of the former country. He also gives a tabular view of the distribution of the 193 phanerogams and four vascular cryptogams hitherto collected, and indicates their possible migrations. His table exhibits the following extensions: Arctic America, Greenland, Iceland, Jan Mayen, Spitzbergen, Bear Island, Scandinavia, Arctic Russia, North Siberia, and the Asiatic coast of Behring's Sound. Out of 193 species, 133 are common to Greenland and 113 to Behring's Strait. The plants new to science are: *Colpodium humile*, *Calamagrostis Holmii*, *Glyceria tenella* var. *pumila* and a hybrid willow—*Salix arctica* × *polaris*; and those new to Novaia Zemlia: *Cineraria frigida*, *Potentilla emarginata*, *Epilobium alpinum*, *Draba repens*, *Ranunculus affinis*, *Alsine biflora*, *Carex incurva*, *C. lagopina*, and *C. hyperborea*. The predominating natural orders are: Gramineæ, 31 species; Cruciferæ, 21; Cyperaceæ, 20; Compositæ and Caryophyllaceæ, 14; Salicineæ, 13; and Saxifragaceæ, 10. Petaloid monocots are limited to *Allium sibiricum* and *Lloydia serotina*; no orchid having been found.

As already indicated, the literature relating to the flora of Spitzbergen and Greenland is voluminous; and although some of it is rather earlier in date than I proposed to attempt to deal with, it seems desirable to make some reference to it, because there is an intimate connection between that and the later contributions; Nathorst and Warming having entered into a critical dispute on the age and origin of the flora. Nathorst's original work in Swedish (8), of which there is a German abstract (9), is a very elaborate performance; the local and general distribution of the plants being most fully tabulated, in order to prove the direction of migration. The author's principal conclusions may be given in all brevity, and without comment. First he points out that the [vascular] flora of Spitzbergen is richer than that of any other country in the same latitude; and many of the

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species are so rare and local that he predicts the discovery of several more species. It should be remembered that the eightieth parallel traverses the north-east island. He was of opinion, too, that most, and probably all of the Spitzbergen plants, have migrated thither during the post-glacial period. About seventy-five per cent. of the vascular plants are described as flourishing perfectly and producing seeds, and these, it is assumed, reached the islands before the remaining twenty-five per cent., which consist mainly of bog and sea-shore plants. The migration of these plants into Spitzbergen was, probably with few exceptions, overland, as Spitzbergen and Novaia Zemlia were connected with Russia and Scandinavia, the connection having since subsided. Nathorst further argues that there have been none but accidental exchanges between the floras of Greenland and Spitzbergen in post-tertiary times. This argument is further developed in several articles (10) in which the author analyses the flora of Greenland in detail; and Warming (11) contests in detail the correctness of Nathorst's theories. Schenk (12) gives a short account of some fossil woods from Green Harbour, figured by Heer (13), which Schenk regards as jurassic rather than tertiary. The interesting fact connected with this is the former existence of arboreous vegetation in so high a latitude as 78°. I can only refer to the reports on the botanical results of the last British Polar Expedition (14), and Lieutenant Greely's Expedition (15); and I merely introduce them in order to render the bibliography more nearly complete for purposes of comparison. Mr. Hart's work is a most valuable synopsis of facts.

Proceeding southward into the more open ocean, we come to the island of Jan Mayen in latitude 71°, and about 150 miles from the coast of Greenland. It is some thirty nautical miles in length, and rises in the north-east to a height of between 6000 and 7000 feet. The central part is a narrow neck of land connecting the northern and southern expansions. Drift-wood Bay, on the eastern side of this neck, received its name and is remarkable for the quantity of drift-wood found there. Until the Austrian expedition

visited Jan Mayen, next to nothing was known of its botany. A Norwegian expedition visited the island in 1877 and collected the following plants : *Ranunculus glacialis*, *Cerastium arcticum*, *Draba corymbosa*, *Cochlearia grœnlandica*, *Halianthus peploides*, *Saxifraga cæspitosa*, *S. nivalis*, *S. oppositifolia*, *S. rivularis*, *Oxyria digyna* and *Catabrosa algida*. Dr. Fischer, of the Austrian expedition (16), collected a considerable number of cellular cryptogams, and added the following vascular plants to those enumerated above : *Ranunculus pygmæus*, *Cardamine bellidifolia*, *Draba alpina*, *Silene acaulis*, *Saxifraga cernua*, *Taraxacum officinale*, *Mertensia maritima*, *Salix herbacea*, *Koenigia islandica*, *Polygonum viviparum*, *Luzula arcuata*, *Poa alpina*, *P. flexuosa*, *Festuca ovina*, *F. rubra*, *Cystopteris fragilis* and *Equisetum arvense* ; making a total of twenty-eight species. As only certain localities were botanised, it may be that a few more species yet remain undiscovered ; but there is no gainsaying the extreme poverty of the flora, as compared to that of Spitzbergen ; and all the species are of wide distribution. Some of them are very rare in the island, or only represented by scattered individuals. The five species of *Saxifraga* are among the most generally dispersed plants, and *Ranunculus glacialis* is the showiest of all. The last bedecks the Alps of Europe as well as the arctic regions of the Old World and America. Dr. Fischer made a collection of drift-woods, which have been worked out (17) ; but I must be content with giving the reference.

Since the appearance of Grœnlund's "Flora of Iceland" in 1874 (18), in which he gave a sketch of the history of botanical discovery in the island, there have been several important contributions, notably one by himself (19) dealing with the composition and general geographical distribution of the elements of the flora. A critical examination of the work of his predecessors led to a considerable reduction in the total number of species believed to inhabit the islands ; the result being 340 species of phanerogams, and 26 species of vascular cryptogams. Not one species is peculiar, and out of a total of 366 species, 360 are common to Scandinavia proper. Fifty-nine natural orders are represented ;

twenty-one of them by only one species each. Brightly-coloured flowers are not wanting, though in number of species those having inconspicuous flowers largely predominate. Thus, of the Cyperaceæ there are 41 species; of the Gramineæ, 36; and of the Juncaceæ, 18. With the exception of the Cruciferæ (21) species, all the others are below 18. Various other botanists have subsequently taken up the subject, and Grœnlund's work has been traversed, criticised, and amended in details. An English summary of this exists (20). There is also a further contribution (21) by a native of Iceland on some new or rare vascular plants from the island. The latest English summary (22) of the flora is an interesting contribution to this literature, though I believe it is little known. The author gives the results of his own collections and impressions, both botanical and entomological, and many are of far more value than one ordinarily meets with, especially those relating to the predominance of certain plants in certain districts; the colour in the landscape; the characteristic alpine, moor, and marsh plants; the plants—*Thymus Serpyllum* and *Parnassia palustris*, that flourish in closest proximity to the hot springs and steam of the geyser; and the plants that are generally distributed in the island, illustrated by comparisons with the conditions and phenomena in other countries. He gives, on the authority of Mr. Arthur Bennet, the total number of species of vascular plants at about 428; but the estimate depends largely upon the view taken of specific limits. Mr. Walker himself collected only eighty-two species; but he separately reproduces the list of 477 species enumerated in Baring-Gould's book on Iceland—a list evidently requiring some revision. Mr. Walker's observations on the entomology of Iceland are equally interesting, especially in relation to the fertilisation of flowers. He states that the leading characteristics are: Total absence of butterflies and *Orthoptera*; *Neuroptera* only represented by *Phryganidæ*; and most of the moths are of a dusky colour, in harmony with the lichens and rocks. Moths and Diptera, he adds, appear to take the place on flowers that butterflies and Hymenoptera do in Britain.

For the latest account of the vegetation of the Færoes, we are indebted to two English ladies (23). Only three of the islands were explored, namely : Stromoe, the largest, which is twenty-seven miles in length and seven miles in breadth ; Naalsoe and Osteroe. Rostrup (24) records 307 flowering plants from the islands, only five of which do not, on his authority, occur in the Scandinavian peninsula, namely : *Alchemilla fissa*, *Anagallis tenella*, *Myosotis repens*, *Scilla verna* and *Carex Lyngbyei.* Further, only ten of them are not found in Britain, and these are almost all strictly alpine in character. As in St. Kilda, trees are entirely absent, and the shrubs of the dwarfest habit. Miss Copland and her companion collected only about a third of the plants, but their sketch of the general character and aspects of the vegetation is most interesting.

The flora of St. Kilda, the most westerly specks of land in Great Britain, is still imperfectly known, though there is an interesting recent contribution to the subject (25). St. Kilda is not so familiar that I need apologise for giving a few facts concerning the group ; for there are several islands. They lie between fifty and sixty miles west of the Outer Hebrides. The largest island, bearing the name given to them collectively, is about three miles long by two broad, and the highest point some 1220 feet above sea-level. So far as the botany is concerned, no species has been found on any of the smaller islands that does not occur on the main island. There is not a native tree, and the shrubby vegetation is limited to such plants as *Vaccinium Myrtillus*, *Erica cinerea*, *Calluna vulgaris*, *Empetrum nigrum*, and *Salix repens ;* consequently, the number of species comprising the flora is very small. Yet there is pasturage sufficient to support considerable flocks of sheep. It includes in its composition over a dozen kinds of grasses, common thyme in abundance, and white clover. The total number of species of vascular plants recorded is 120, several of which, however, are exceedingly rare, or only grow in the barley and oat crops, thus bringing down the probably indigenous species to about 100 ; a number that is equalled on an acre of ground in the South of England. Genuine alpine plants

are almost wholly wanting: *Silene acaulis*, *Saxifraga oppositifolia* and *Oxyria digyna* are the nearest approach to this class of plants.

A catalogue of the phanerogamic plants of Madeira and Porto Santo (26), not included in the unfortunate Lowe's unfinished flora of Madeira and the neighbouring islands, will be found useful, though it only causes us to lament the more the untimely end of that author. This catalogue contains naturalised plants and a few notable cultivated ones, with remarks on the local distribution of the indigenous species, many of which are exceedingly rare. The following are described as new:—

Koniga arenaria, *Scrophularia Moniziana*, *S. Johnsoniana*, *S. maderensis*, *S. oblonga* (name only), *Romulea juncea*, *Tinantia fallax*, *Potamogeton cuprifolius*, *P. Machicanus*, *Phalaris altissima*, and *Sesleria elegans*.

The Canary Islands have furnished facts and figures for the phytogeographer from almost the earliest investigations of the distribution and migration of plants. Humboldt and other fathers of the science drew largely at this fountain; and their labours together with Webb and Berthelot's great descriptive work might have been considered exhaustive, yet Dr. Christ (27) has collected data for one of the most interesting of recent essays on geographical botany. Dr. Christ's work is the result of personal observations, and it is very much more than a statistical exposition of the components of the flora. Biological phenomena receive due attention, as is exemplified by the discriminating manner in which he describes the vegetation as distinguished from the flora. This work may be profitably studied in connection with Dr. Balfour's flora of Socotra (28), in which the author draws some striking comparisons of the relationships between the floras of these two distant insular regions. Primarily, Dr. Christ insists upon a close relationship between the floras of the Azores, Madeira, the Canaries, and the Cape Verd Islands, and treats them as parts of an intimately connected whole. Apart from the last, there is no doubt that the predominating elements are much the same in these four groups of islands, which are scattered through twenty-five degrees of latitude from 15° to 40°. But although there is

a large temperate element in the Cape Verd Islands, considering their tropical situation, and a subtropical element in the Canaries, there is hardly that homogeneity throughout to warrant the conclusion that they are actually separated fragments of one and the same flora, as distinguished from the flora of the nearest continental regions. It is true that, taking the Canaries as the centre, and the north and south groups as outliers, there are very evident connections, which Dr. Christ finds even "really surprising" in the case of the Cape Verd and Canaries; but before entering into details of this part of the question it may be well to give some of Dr. Christ's facts and conclusions. It should be remembered that the Canaries are the nearest to the continent of the four groups of islands or archipelagos—Azores, Madeira, Canaries, and Cape Verd; and the islands Lanzarote and Fuertaventura are much nearer than the rest of the Canary group, being only about 1° distant. The peculiar element in the flora of the latter being comparatively small, Dr. Christ would prefer designating them continental in contradistinction to the five western essentially oceanic islands. But this is an unnecessary and untenable distinction, for after all none of these islands are oceanic in the sense that St. Helena, Tristan da Cunha, Amsterdam, Rodrigues, the Galapagos, and the Sandwich Islands are. Nor is the endemic element of the same pronounced character as in most of the islands named, being more comparable to the differences existing in some continental areas, in places no more distant from each other than the Canary Islands are from each other and the mainland. Confining ourselves to the African flora, a more strongly marked differentiation will be found to exist in South Africa, within an area extending over four or five degrees of longitude or latitude. Indeed, a great deal too much has been made of the assumed extreme differentiation exhibited by insular floras as opposed to continental floras. Even the distinctive habit of the succulents and the half-shrubby plants that replace the truly herbaceous element in more humid climates, on which considerable stress is laid by Dr. Christ, is not greater than in South Africa.

Returning to the flora of the Canaries, Dr. Christ dis-

tinguishes three zones, or regions, as he denominates them; namely, the coast, cloud, and uppermost regions. The coast region includes the "barrancas" or ravines, and the whole of the cultivated part of the country ranging from the sea-shore up to an elevation of about 3000 feet, and consequently, it is here, too, that the colonised plants are found. Contrary to what has happened in St. Helena and many other islands, introduced plants have made comparatively little way in the Canaries, and, indeed, in the Azores and the Cape Verd Islands—that is to say, beyond the coast region. It is true that the formerly extensively cultivated cochineal cactus (*Opuntia*) has over-run the subtropical part of the country; but what was formerly the chief source of income now remains as a scourge; being one of the few plants capable of competing with the indigenous vegetation. The endemic palm, *Phœnix canariensis*, the aloe, the dragon's blood tree, and the cactus-like shrubby and arboreous euphorbias are conspicuous features in the landscape. *Tamarix canariensis* is the prominent shrub or small tree of the sandy sea-shore; but the endemic *Plocama pendula*, a Rubiacea, with the habit of a casuarina, and various cactus-like species of *Euphorbia*, are the most striking and characteristic of the inhabitants of the succeeding rocky country. The "Cardon," *Euphorbia canariensis* forms dense clumps five or six feet high, consisting of numerous thick, fleshy, angular, prickly stems springing from the same root. But the commonest and tallest of this genus is the "Tabayba," *Euphorbia Regis Jubæ*, occupying the driest situations in the Western Canaries, sometimes growing to a height of twenty-five feet, and forming densely-branched hemispherical masses. Independently of the paragraphs in the essay cited, Dr. Christ has a special article on the Canary species of Euphorbia (29). The famous "Dragon's Blood Tree" (*Dracæna Draco*) is, or rather was, an even more striking feature in the endemic vegetation of the barrancas.

With regard to the great age formerly attributed to this colossal monocotyledonous tree (*Dracæna Draco*), Dr. Christ's measurements confirm the opinion expressed by the writer (30). There is no doubt now that it is of exceedingly rapid growth. A trunk of one growing at Icod los

Vinos was 9·5 metres in circumference at 2·5 metres from the ground in 1857; and this had increased to 11·7 metres in 1884. Shrubby and half-shrubby Compositæ, and numerous species of the boragineous genus *Echium* abound; but the fleshy-leaved Crassulaceæ give character to the vegetation. The latter mostly belong to the type having the leaves arranged in dense, often large, rosettes, from which rise the leafless inflorescences. Nowhere else in the world is there such a concentration of this class of plants; no fewer than fifty-two species being enumerated by Christ, mostly belonging to the genus *Sempervivum*. *S. tabulæforme* forms rosettes as much as fifteen inches across; and, contrary to what is generally supposed, the roots of these succulent plants penetrate the fissures of the rocks to an incredible depth in search of moisture. *Statice* is another prominent genus of perennial duration, being represented by about a score of species. Apart from weeds of cultivation, there is very little truly herbaceous vegetation, and bulbous plants are rare.

The "cloud region" is a zone above cultivation, which is almost constantly enveloped in clouds, engendering a green or leafy vegetation. It is the zone of the laurel forest, consisting of *Persea indica*, *Laurus canariensis*, *Oreodaphne fœtens*, and the much rarer *Phœbe barbusana*. With the exception of the last, which only extends to Madeira, Dr. Christ records these laurels as common to the Canaries, Madeira, and the Azores; but this is probably a slip so far as the *Oreodaphne* is concerned. They are, however, all confined to the Atlantic Islands. *Erica arborea*, *Myrica Faya* and *Pteris aquilina* are the three predominating species in the undergrowth; the two first being both arboreous in the forest itself. Endemic species of *Ilex*, *Myrsine*, *Notelæa*, *Clethra*, *Arbutus*, and *Visnea*, are other noteworthy elements of the vegetation of this zone.

Teneriffe alone has a subalpine region above the forest, with a dry, scorching climate and a thinly-scattered vegetation, consisting largely of *Spartium supranubium*. Herbaceous plants are sparse and of a greyish hue, such as *Viola cheiranthifolia* and *Silene nocteolens*.

Taking Sauer's estimate of 1226 (31), as representing

approximately the number of species of vascular plants growing wild in the Canaries, Dr. Christ would deduct upwards of a third (420) as not being really indigenous; in other words, as not belonging to the original flora. This leaves 806 species, of which 414 are endemic in the Atlantic Islands; and 392 are also continental. About the same percentage obtains in the West Indies; but it is much higher in Mexico, South Africa, and East and West Australia. Still the peculiarity of the Atlantic Islands flora is rather in the habit of the endemic species than in their relative number. By habit I mean vegetative characteristics, which give the endemic plants a vital energy sufficient to enable them to hold their own against all intruders. On this point Dr. Christ has collected a number of highly interesting facts. Grisebach (32) was of opinion that the endemic flora of the Canaries was dying out, and would soon be exterminated by the more vigorous colonists from the continent. Happily, says Christ, this is an error. C. Bolle, the most experienced and exact among Atlantic botanists, pointed out long ago (33) that the native flora would indefinitely survive in spite of cultivation, and the protection afforded by man to introduced plants. The apparently proscribed plants were constantly gathering new strength to recover the lost ground. "In short," Bolle says, "it (the endemic flora) is everlasting, indestructible; and vast tracts of country not forfeited to cultivation are still exclusively left to it." Twenty five years later Dr. Christ found everywhere full confirmation of this statement. In the struggle for existence, he asserts, the local conditions are all in favour of the native plants and against intruders; and so long as the present conditions continue, so long will the present flora flourish. I have already mentioned the great development of the root-system of the succulent plants growing on rocks; and I may add that it is to the great vegetative capacity of the native plants generally that Dr. Christ ascribes their power of resistance to foreign invasion. This applies more especially to the half-woody plants, belonging to genera whose continental species are mostly herbaceous. The huge clumps of rosettes of *Sempervivum*, and the dense hemispherical tufts of *Echium* and *Statice*

are examples of what is meant. Associated with this almost unlimited vegetative power there is a comparatively rare production of flowers, and the flowers are usually small or very small. But the inflorescences, which only succeed each other after long intervals, are commonly very large and very many-flowered; the outcome of accumulated vital force. Another peculiarity of the Canary flora as an insular flora, and, as Dr. Christ suggests, a proof that it is not the dying-out remnant of a richer flora, is the relatively small number of monotypic genera, and genera poor in species. In this respect, he says, it is quite exceptional; but the Hawaiian and Galapagos floras also include a number of genera represented by a long series of species. Christ enumerates twenty-six endemic, monotypic genera, or sections of genera, as against twenty-three endemic genera, or sections of genera of more than one species, and fifteen continental genera represented by three or more endemic species.

Pursuing his studies of the Canary flora, Dr. Christ has published (34) some further contributions to the flora, including descriptions of many new forms, mainly from Webb's unpublished manuscript. There is nothing very remarkable among these additions; but it may be worth noting that Christ describes the plant issued by Webb under the manuscript name, *Todaroa montana*, without any reference to the fact that Bentham and Hooker (35) had discovered that *Todaroa* was a slip of the pen for *Tinguarra.* Appended to the *Spicilegium* is a *Catalogus Plantarum tam Canariis propriarum quam has insulas inhabitantium sed etiam in insulis Azoricis, Maderensibus et Gorgadensibus nec alibi crescentium.* It includes 477 species, with their distribution in the four groups of islands—in fact, the endemic element of the Atlantic islands, as represented in the Canaries.

Since the appearance of Dr. Christ's papers on the flora of the Canaries, Dr. C. Bolle has published a more detailed list of the plants of the islands, Lanzarote and Fuertaventura (36). He fully agrees with Christ in describing the vegetation as very different from that of the other five islands more distant from the continent. Bolle's list includes colonised plants; but it is a second later paper (37) that more especially claims our attention. This is an historical sketch of

the vegetation of these islands, and an analysis of its composition and relationships. The principal feature in these islands was, and is, the groves of date palms. Bolle regards all the date palms of these islands as the true date of North Africa, *Phœnix dactylifera*, and describes its occurrence in the following words: "Ubiquitaria fere locis idoneis. Sylva miranda, saharienses oases referens, in convalle Rio Palmas. Num *Phœnix canariensis*, Recentiorum quoque in Purpurariis indigena sit, adhuc dubium." On the other hand, Christ writes as though the common wild palm was the *P. canariensis* (*P. Jubæ*), and adds that the continental *P. dactylifera* is extensively cultivated in the islands, where it yields excellent fruit. It is now generally admitted that the indigenous insular form is specifically distinct from the continental one; yet much uncertainty exists in the various attempts at discriminating the two.

Bolle's analysis of the flora shows an endemic element of thirty-five species; that is, peculiar to these two islands. Fourteen of these are exceedingly local. He further distinguishes forty-six species as belonging to the characteristic Canary type, and twenty-six to the Sahara type. His catalogue comprises upwards of 400 species, including colonists; and *Ononis Christii*, *Lotus erythrorhizus*, and *Plantago Aschersonii* are described as new.

Mr. Krause's sketch of the flora of St. Vincent (38) affords material for a comparison of the Cape Verd and Canary floras. The Cape Verd Islands are upwards of 300 miles from the mainland, and in St. Vincent there is a range of hills culminating in the eastern part of the island, in the Green Mountain, at an altitude of about 2500 feet, with cultivation to the summit. There is, in a sense, a wet and a dry season, but prolonged drought is not uncommon. Of arboreous vegetation there is none, and the real shrubby vegetation consists almost entirely of *Tamarix senegalensis* and *Euphorbia Tuckeyana*; the former, rarely ten feet high, in thickets, on the coast and in the sandy valleys; the latter, sometimes as much as six feet high, common and scattered all over the island, from 200 feet upwards.

Krause's list, compiled from all available sources besides his own collection, comprises only 183 species of vascular

plants; one-third of which he regards as introduced plants. Of the remaining 121 indigenous species many are endemic; but *Tornabenia* (Umbelliferæ) is the only endemic genus; indeed, the only one peculiar to the Archipelago. On the hillsides, from an altitude of about 1300 feet to the summit, the shrubby *Euphorbia Tuckeyana* forms thickets, in some places as much as eight feet high, but usually not more than three. Prominent among the endemic plants associated with the *Euphorbia* are: *Echium stenosiphon*, *Sarcostemma Daltoni*, *Sempervivum Webbii* and *Lavandula rotundifolia.* Altogether, about two-thirds of the species inhabiting the uplands are endemic.

With regard to the affinities and origin of the flora of the Cape Verd Islands, Krause in the main agrees with Christ; but he carefully distinguishes between the two questions. He finds the nearest affinity in the southern half of the Canaries, relying largely on the *Euphorbia* and *Dracæna* elements. *Dracæna Draco* has disappeared from the island of St. Vincent; but is still said to exist here and there in the mountains of St. Nicolao and St. Antonio. But the total absence of the *Genisteæ*, *Laurineæ*, *Chrysanthemum*, *Rhodorhiza*, and *Phœnix*, and the comparative rarity of other characteristic Canary plants, points rather to community of origin than to community of descent. Neither Christ nor Krause will admit of a former land connection of the four groups of Atlantic islands; and the latter regards a former connection of the Cape Verd Islands with the continent as highly improbable. Yet, as before stated, Dr. Christ regards the floras of the islands from the Azores to the Cape Verd as more nearly related to each other than separately to any part of the continental flora. Nevertheless, I agree with H. C. Watson (39) that the flora of the Azores corresponds closely to that of South-western Europe. His list of plants, which occur in the Azores, but not in Europe, and are also common to Madeira or the Canaries, or both, is a very small one, and, as he himself suggests, might be considerably reduced, as it includes a number of plants of wide distribution, among them some that are only colonists in any of the Atlantic islands.

Similarly, I would say that the flora of Madeira, Canaries, and Cape Verd Islands is African, with strong affinities in Eastern North Subtropical Africa.

On the opposite side of the Atlantic to the Cape Verd Islands is the Fernando Noronha group, situated in about 3° 50′ S. lat., and nearly 200 miles distant from Cape San Roque, Brazil, which was discovered by Amerigo Vespucci in 1503. Several naturalists of note have touched there, and collected a few plants, including Darwin in the *Beagle* and Moseley in the *Challenger;* but it was not until 1887 that the islands were thoroughly botanically explored. Aided by a grant from the Royal Society, Mr. H. N. Ridley and Mr. G. A. Ramage spent about six weeks on the islands in that year, and the former published (40) the botanical results of the expedition. These results were somewhat disappointing, because Fernando Noronha had a reputation for insularity which it did not deserve. This was doubtless owing to the fact that both Darwin and Moseley's very small collections of plants contained previously undescribed species ; yet, as it turned out, they had happened to put their hands upon the most striking of the few endemic plants, and Mr. Ridley had few novelties to add. Indeed, there was little to add concerning the general character and origin of the vegetation to that given by the writer (41) in 1884. It is there stated that the flora is quite tropical American, with no greater infusion of peculiar species than would be found in a similar area on the mainland. The new species exhibited no striking characteristics, and it was not probable that further exploration would lead to the discovery of a specially insular endemic element. Nevertheless, Mr. Ridley designates the group as "oceanic," though he had only a few, mostly critical species of well-known genera to add to those previously known. Who first regarded the group as belonging, in relation to the origin of its flora, to the same category as St. Helena, I have not been able to ascertain ; but I find that Dr. Ihering, in an article that is certainly deserving of perusal (42), attributes it to Wallace, and goes so far as to assert that the latter had taken its flora to be primarily of African origin, conveyed by oceanic currents and other means. This seemed to me so

utterly improbable that I wrote to Dr. Wallace on the subject, and he replied to the effect that it must be a mistake on the part of Dr. Ihering. Returning to the composition of the flora, the chief novelties, assuming that they do not exist on the mainland, which is by no means certain, are: *Oxalis Noronhæ*, *Combretum rupicolum*, *Erythrina aurantiaca*, *Cereus insularis*, *Bumelia fragrans*, *Solanum botryophorum*, *Pisonia Darwinii*, and *Ficus Noronhæ*. Of course, there is absolutely nothing in the composition of the vegetation to suggest an African origin, and Dr. Ihering may have intended Tristan d' Acunha when writing Fernando Noronha. The exceedingly meagre vegetation of South Trinidad (20° 30′ S. lat., 29° 22′ W.) includes *Asplenium compressum*, a fern only known elsewhere from St. Helena; and the genus *Achyrocline* (Compositæ), which is both African and American, is represented by an endemic species. It may be mentioned incidentally that Dr. Ihering enters somewhat fully (42) into the origin of the southern insular floras; and he also discusses the possible and probable agents of dispersal, arriving at much the same conclusions as the writer, though he does not appear to have known of the existence of the reports on the botany of the *Challenger* Expedition. Turning to Ascension Island, it was not to be expected that after the investigation of such keen botanists as Hooker, Burchell, and others, any species had escaped detection, yet an American expedition adds three proposed new species (43), namely, *Rubus nanus*, *Asplenium ascensionis*, and *Nephrodium viscidum*. The author was manifestly unaware of the extent to which plants were introduced into the islands from Kew and other places, during the period that cultivation was attempted, for the purpose of supplying ships with vegetables, and even for more ambitious schemes, such as Cinchona planting. There is hardly a doubt that the *Rubus* is a descendant of an introduced species. This view is confirmed by the statement that "it appears to be a very distinct and pecular species of a genus not otherwise represented in the flora of the islands of the South Atlantic". There is a fragment of the *Asplenium* in the Kew Herbarium, collected by Don on his journey to

Sierra Leone; and the material of the *Nephrodium* was hardly sufficient to base a new species upon. Mr. Watson expresses regret that the opportunities of the expedition for exploring the island were not greater, "because they might have considerably increased the number of indigenous species known"; but, of course, this is most improbable, as the island has been as thoroughly explored as any island in the world.

Concerning the very small remnant of the original vegetation of St. Helena, no further information has been recorded during the last decade. It may be mentioned, however, that Penhallow's "Flora of St. Helen's Island" has, by some oversight, been confused with St. Helena in the Atlantic (44).

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FOLDS AND FAULTING: A REVIEW.

PART I.

TO those who are interested in the deep and complex problems geology is called upon to solve, the triennial geological congresses afford a special means of obtaining fresh light and knowledge, under the guidance of leaders whose life-work has been carried out in the country, the structure of which they are called upon to explain. Men holding the most varying views are brought to examine the same facts from their different standpoints, and the discussions which result from their contact lead to conclusions of permanent value and wide-spreading influence.

The meeting of the latest congress at Zurich, the subsequent traverses across the Alps, and the publication of the Livret-guide explanatory of these excursions, have directed the minds of a large number of geologists to those movements of the earth's crust, which have given rise to that splendid range of mountains, whose detailed structure and scenery we were called upon to examine.

In the earlier part of the century the most varied theories had been enunciated in regard to these inequalities of the earth's surface, the facts of mountain structure being little understood. Volcanic agency was made largely responsible for these upheavals ; the wedge-like intrusion of igneous masses, the liftings of an internal igneous nucleus, or the sinking of the ground by the removal of volcanic material, being all called into requisition to account for the phenomena. Simple upward pressure stood in sharp contrast to horizontal or lateral compression, and around these rival standpoints raged a combat not less severe than those which in later years have divided the geological world.

The conceptions which have in the last decade gained the ascendency in all probability first had their rise in the

masterly work of the Brothers Rogers, when studying the Appalachian range. The full results of their labours were brought before the British public by Professor H. D. Rogers in 1857 (1), and it is interesting therein to note the great advance already made towards a true knowledge of the elements composing a mountain range. These observers early recognised that every tract of the earth's crust which had undergone upheaval had a wave-like form, and that faults were merely separated, disarranged portions of what were originally continuous undulations; more than this—that in regions of great disturbance the strata were arranged in belts of parallel waves, the region of maximum disturbance being marked by a closer folding of the same. Each primary undulation itself might be thrown into secondary waves, which, though not necessarily parallel to the primaries, nevertheless preserved a constant parallelism between themselves, and a third class might also be produced, giving rise to the so-called rolls of strata. But these were not merely simple waves of undulation, for the folding changed its character from *symmetrical* flexures, dipping equally on both sides, to *normal* flexures, where the dip was steeper on one side than the other. Finally, in regions of greatest disturbance the flexures are overfolded, there being an actual inversion, or doubling under, of the steeper side of each curve. Thus the lines bisecting the half curve, the axis-planes, usually dip at a very low angle, and towards the region of maximum disturbance.

Further, they recognised that a close series of plications may appear as one of simply conformable deposits, especially in cases where slaty cleavage is set up. Failure to recognise this principle has in subsequent years given rise to many difficulties, and complicated theories of migration have been invented to account for the repetition of zones of the same fossil at different levels in the same group of strata.

Unlike Elie de Beaumont, they came to the conclusion that crust waves were not straight only, but also curvilinear, and that flexures grade away from the districts of maximum disturbance and contortion, being highly plicated in those

regions, and gradually opened and flattened out as they recede from such a centre.

Whilst noticing cases of more ordinary transverse faulting, they laid special stress on the occurrence of the greater longitudinal overthrusts parallel to the main anticlinal axes, which, owing to more or less compression, had snapped and given way in the act of curving. It was found in South-west Virginia that the youngest stratum first disappeared, the older ones following in succession, until finally the Lower Limestone of Cambrian or Lower Silurian age comes to absolutely overlie the Carboniferous Limestone. The strata thus engulfed represent a thickness of over 8000 feet, and the uninverted side of the wave had been driven eighty miles over the more inverted portion.

Those who are acquainted with the present condition of this discussion will at once appreciate the solid basis upon which the above conclusions were founded, and it is equally to the credit of Professor Rogers that he came over to Europe to test the accuracy of his conclusions in those regions where similar results might be expected. The Devonian strata of the Rhine, as also the Jura range, again showed the passage from broad waving of the strata to that of more complicated flexuring, but, above all, in the Alps he found the absolute comparison he was in search of. He says: "We behold an exact counterpart, in the stratification or structure of a single flank of the Alps, of that folding with inversion, which characterises the Appalachian chain, or that of the Ardennes, a single side of the Alps being the equivalent of the whole of either of these zones; it consists, that is to say, of an undulation in one direction". He concluded that the flexure could only be due to the actual pulsation of fluid matter beneath the crust, propagated in the manner of great waves of translation from enormous ruptures occasioned by the tension of elastic matter. The various features observed were ascribed to the combination of an undulating and a tangential movement, accompanied by an injection of igneous veins and dykes into the fissures occasioned by the bending.

Sir Henry de la Beche (discussing Professor Rogers'

hypothesis) (2) pointed out that the conditions observed were perfectly compatible with the assumption that lateral pressure, *viz.*, that of masses of the earth's crust against other masses along lines of fracture on the surface, had been the cause of these phenomena. About the same time, Sir R. Murchison (3) had clearly seen that, in those parts of the Alps where crystalline overlay unaltered sedimentary strata, this result would be due to "one enormous overthrow, so that over the wide horizontal area, the uppermost strata, which might have been lying in troughs or depressions due to some grand early plication, were covered by the lateral extension over them of older and more crystalline masses".

In 1856 Professor Nicol (4) hinted at the possibility of such conditions having likewise existed in the Scottish Highlands. "The termination of the Quartzite period seems again to have been marked by convulsions. To these we must refer the action by which the higher portions were converted into gneiss, or *this gneiss* if a pre-existing rock forced over the quartzite."

Two years later, Sir R. Murchison (5), examining the same country, was led to a conclusion which for twenty-five years proved a stumbling-block to the larger number of English geologists. To him the succession in North Scotland appeared as a perfectly simple upwardly ascending series, the upper quartzite and upper gneiss being not only apparently, but in reality, younger than the Durness limestone they seemed to overlie. To this view the most distinguished amongst our English geologists rallied, and Professor Nicol's protest in 1861 (6) carried no conviction to the minds of the majority. Undoubtedly he weakened his position by doubting the existence of gneissose rocks above the limestone series, and assuming that simple faulting might account for all the phenomena observed. Nevertheless, in justice it must be allowed that he recognised that "a comparatively very small amount of inversion and extrusion of older crystalline masses will suffice to explain any of the Scottish sections, even as drawn and described by the advocates of an overlying younger gneiss". After

giving an example of inversion and intrusion, he adds: "Until some rational theory is produced of the mode in which an overlying formation, hundreds of square miles in extent, and thousands of feet in thickness, can have been metamorphosed, whilst the underlying formation, of equal thickness, and scarcely less in extent, has escaped, we shall be justified in admitting inversions and extrusion equal to those in the Alps".

If somewhat vaguely expressed, it is evident that Nicol's thoughts were already tending in the direction of a truer solution, but the answer given by Murchison and Geikie (7) appeared so conclusive and overwhelming that further discussion was silenced. Pure stratigraphy had triumphed, and tectonic geology slumbered, in so far as the British Islands were concerned. Meanwhile, on the Continent and in America, materials were being accumulated, destined eventually to lead to conclusions of the highest importance; but before the new views could be brought into prominence, one of the conflicting hypotheses had first to gain a commanding position. Only with the abandonment of Von Buch's hypothesis of elevation by Plutonic upheaval, and the recognition that Elie de Beaumont's brilliant theories of mountain structure were untenable, could lateral pressure due to contraction of the earth's surface become a serious factor in the discussion of foldings. Hall, De la Beche, Prevost, Pratt and Fisher had been largely instrumental in bringing about this valuable result, whilst Daubrée had attacked the question from a practical standpoint, and had carried out a large number of experiments having a most important bearing on the point at issue. In 1875 Dana, adopting a line of thought parallel to that already pursued by Babbage and Herschel, pointed out the important relation existing between mountain formation and the previous deposition of thick layers of sedimentary deposits, the position of the future mountain range being first marked out by the slow formation of a geo-synclinal, the deposition of sediment being concurrent with the progress of depression. As the geo-synclinal descends, a geo-anticlinal must be formed on one side, or possibly on both, but in the Appala-

chians the first condition seems alone to prevail, the lateral pressure having acted unequally from the oceanic and from the continental sides.

When a vast thickness of strata has thus been depressed below the surface, the bottom of the geo-synclinal eventually becomes weakened by the heat rising from below, and, partially yielding to the pressure, the rocks become displaced, upturned, folded, and fractured.

In the same year Suess published his volume, *Die Entstehung der Alpen*, which was destined to at once raise the whole discussion to a higher level than it had ever before attained. The labours of Favre and Lory in Switzerland, Beyrich in Bohemia, Abich in the Caucasus, Stoliczky and Medlicott in India, Von Richthofen in the Carpathians and China, not to mention a number of other observers, were laid under contribution to assist in building up those generalisations, the results of which we enjoy to-day.

Accepting as his theoretical basis the contraction of the earth's surface, Suess pointed out that the Alps formed part of one enormous belt encircling the globe, and were therefore not merely local occurrences, but component parts of one vast movement. Further, that not merely the central crestal ridges should form the subject of discussion, but that a whole region or series must be taken into account. As a result of his observations, he is of opinion that there are directions of mountain flow, the forward movement being marked by the overlapping or overthrust of beds belonging to the Alpine system, over those which have not shared in the movements resulting from contraction; whilst, in the rear of the mountain-axis, great faults and considerable fractures are produced, these, as in the Apennines and Carpathians, occasionally giving rise to important lines of volcanic eruption.

Not only, therefore, are many of the most magnificent mountain systems asymmetrical, but they have also a definite movement, flowing towards, or from, the poles. From the Cordilleras to the Caucasus this flow is uniformly northwards; whilst across the whole of Asia the conditions,

whether stratigraphical or tectonic, are reversed, the principal massifs forming a curve, presenting their convex front towards the south. On this hypothesis, therefore, mountain ranges result from unequal contraction, and are not the products of forces acting equally in opposite directions.

But further, the contracting mass is affected by hindrances, and re-acts upon these in a peculiar and striking manner. After overlapping the old gneiss of the Dôle area and the ancient continental regions of the Black Forest and Bohemia, the younger Alpine series, freed from the resistance of these higher districts, spread over the lower grounds of Galicia, unconformably overlying the older carboniferous beds of that province; Professor Suess, indeed, considers that the north-east and south-west foldings, which can be traced through the Erzgebirge, along the Rhine into Belgium, and over to our own islands, are the result of the action of that powerful pressure which the outstanding sentinels had only apparently successfully resisted.

The discussion as to the transgression of the Cenomanian, both in Europe and Asia, has proved that these earth folds have not been mere local phenomena, but that we are dealing with events of the highest importance in physical geology. It has now been shown that the Upper Cretaceous beds transgress over the older in Northern Europe, Northern Africa, India, and Eastern Asia, and we have also lately learnt that the same holds good for Chili.

At the recent Swiss Congress Professor Steinmann gave an account of the fauna of the Quiriquina beds in that country, and showed that three-quarters of the Ammonites there met with are absolutely identical with Indian types, the Gasteropoda and Lamellibranchiata being also similar to those occurring in the Utatur strata, near Pondicherry, and therefore he deduces that, like these, the Chilian beds must be of Cenomanian age. The similarity extends yet further, the transgression being as strongly marked in these strata as in the Indian type.

The Shore Cordilleras consist of mica schists, granites, etc., and these older rocks are directly overlaid by the Quiri-

quina beds, the Jurassic strata being entirely absent at this point, though present to the north and south. The conclusion forced upon us therefore is, that here we have a remnant of an old continent, or land barrier, which remained when the great depression took place, whose effects are so strongly marked throughout the world.

Whilst the publication of this work by Professor Suess, with its ingenious blending of ascertained fact and well-balanced hypothesis, opened up a new world of activity in geological thought, the labours of Escher, Baltzer, and Heim were laying the foundations of a new structure which was to celebrate the apotheosis of the fold. Commenced by Baltzer in 1873 with the issue of his paper on the Glärnisch, it was consummated by Heim in 1878 in the publication of the *Mechanismus der Gebirgsbildung*, with its wealth of detail and beauty of illustration, which gave a new impulse to geotectonic discussion, and influenced deeply many of those geologists who are now taking a front rank in this particular branch of inquiry.

Professor Heim lays special stress on the importance of foldings in mountain structure. His theory may thus be succinctly set forth. At a certain depth beneath the earth's surface the rocks are loaded far above their power of remaining solid. This pressure is applied in all directions, so that each individual component is equally affected, and even the most massive rocks are maintained in a state of latent plasticity. Should there now ensue a disturbance of equilibrium through the application of a new force, the horizontal mountain-forming compression, there will be then a mechanical transformation in the deeper-seated portions without fracture, but nearer the surface in the more massive materials fracture would result. It follows, therefore, from this that all the foldings we see in the Alps have been formed deep in the earth's crust, and the whole of the strata that overlay them have disappeared under the action of denuding and erosive influences. Ordinary faulting is of the rarest occurrence, and the throw rarely exceeds a few metres, and even then the effect is purely superficial. He points out as a matter of fact that in the younger geo-

logical strata transformation with faulting predominates; whereas, in the older, transformation without fracture becomes more and more common, and gives as a further result the fact that the outer chains consist of Mesozoic rocks with beautiful gentle curvatures, whereas the innermost most ancient ones are much more sharply and irregularly bent.

This, then, is his broad principle: Where rocks have been gradually depressed under a mass of sedimentary strata they will pass more or less readily into that condition suitable for the development of pure folding. Supposing, therefore, 3000 feet of superincumbent strata should suffice to induce the necessary change in clayey materials, it would require a far greater weight to render massive limestones sufficiently plastic to obtain the like result.

In an eloquent chapter (Theil ii., pp. 114-128) Professor Heim attacks a position which had already been proved untenable, and by showing the eruptive rocks of the Alps to be in every case older than the main upheaval, he has effectually dispelled the reasoning which would attribute to them the cause of mountain formation. If there be any who, on the basis of Lawson's study of the gneiss in the Rainy Lake region, regard most of these as of very late intrusion, they have before them a most difficult task should they attempt to apply their theory to the whole of the Alpine gneisses.

The conclusion arrived at by Professor Heim respecting the Alps is decided and clear. He refuses to admit that they have been elevated by eruptive agency; the rocks are older, and have been brought to their present position in a passive manner, and he maintains that eruptive rocks do not produce a mountain chain. He affirms that everything at present known as to the structure of the crystalline rocks composing the Centralmassif agrees absolutely with the conception that they are the arched portion of a fold system of the crystalline earth crust. The crystalline rocks are frequently bent in such a manner at their point of junction with the sedimentaries that they agree more or less closely with them as regards position. The sedimentary rocks,

often in their condition very similar to the crystalline, not only skirt the Centralmassif, but often penetrate it, and form an integral portion of its construction. These are not separated portions embedded, but the remains of closely compressed troughs. The Centralmassifs have not folded the sedimentary chains by their lateral activity, but are themselves zones of the earth's crust which were formerly overlaid by sediments, have since undergone compression, and are now exposed through denudation and erosion. The folding of the Centralmassif is of younger tertiary age, and therefore synchronous with that of the sedimentaries ; any older flexuring could, at most, have been of a very feeble character (*loc. cit.*, p. 178). All the Centralmassifs and all the limestone chains of the Alps have been formed, because the sum total of their foldings represents the force necessary to neutralise a definite tangential compression. The innumerable magnificent ridges and peaks, so varied in form and outline, are the outcome of the same activity and of the same period, no matter whatever may be their materials; they are probably the result of a pressure able to overcome the cohesion of the rocks and producing folds, whereas the remainder of the earth's crust had not shrunk one-hundredth part towards its centre (*loc. cit.*, p. 186).

Having thus established the principle that folding is the main and necessary result of the compression, he proceeds to a study of the folds themselves, these being either normal, with the beds dipping away from the axis plane in the arch, and towards it in the trough ; isoclinal, where both the anticlinals and synclinals are in such a position that the beds are parallel to the axis planes and appear to be concordant in stratification ; and fan-shaped when the strata both in the anticlines and synclines dip towards the axis planes, producing by this means the appearance of the younger rocks underlying the older. Should this latter form become very oblique, either a lying isoclinal overfold or fan-shaped one may be produced, the middle limb of which may be squeezed out. These may pass directly into fold faults, there being no strict boundary between the latter and the overthrust planes.

The Alps consist of about a dozen main folds, but the total number of secondary ones cannot be estimated. Nevertheless, taking the whole of the great movements into consideration, the earth has been reduced by no more than a hundredth of its previous circumference. Pressure may have already commenced its action in Cretaceous or Eocene times, the highest existing fold being in all probability the earliest, and at the time that the outermost post-Miocene Alpine chains were being formed, the inner zones were already undergoing denudation and erosion. Seeing, however, that the Central Alps are higher than the outer ridges, it follows that elevation must have been more rapid than denudation.

We must now call attention to a special case involving the highest conception of a fold as yet submitted to the notice of geologists. We refer to that of the Great Glarner Doppelfalte or Doublefold. Commencing near the Central-massif, west of the Reuss, it continues as far as Ragatz, the northern part of the fold having a length of ninety kilometres, and the southern, one of forty-eight. In the whole of this region, on both sides of a central axis, older beds of Verrucano overlie the younger Eocene, the two being separated by a limestone complex, the Lochseitin Kalk, in which Escher von der Linth, after much wearisome research, found Jurassic Belemnites and Ammonites. The theory propounded supposes that at this point intense folding had taken place in two opposite directions ; the arch has moved forward, whilst the trough has travelled in the opposite direction. The septum, or middle limb, being drawn out owing to the advance of the arch, has at the same time been squeezed out between the arch and trough cores. In the above case, through this movement the whole series from the Permian to the Upper Jurassic is only represented by a highly altered limestone a few metres in thickness.

The above-named two trough limbs are connected deep beneath the surface, and the Eocene core being compressed between the two foldings has itself become enormously contorted and bent. The theory of plasticity already referred

to excludes any supposition of overthrust faulting, and in consequence we are forced to the conclusion that these are really enormous overlying folds travelling in opposite directions. We would lay special stress on the theory here enunciated (see fig. 1), because presently we shall see that exception has been taken to the views here set forth.

The publication of Heim's valuable work, marked alike by beauty of illustration and breadth of theory, undoubtedly gave a great impetus to the study of these tectonic questions, but in justice it should not be forgotten that part of any credit due to a juster appreciation of mountain foldings must be given to Professor Baltzer, who, by his work on the Glärnisch, and subsequently for the Swiss geological

FIG. 1.—Illustrating Heim's theory of the Doppelfalte.

A. Arch Limb (Mesozoic rocks mainly), now denuded away.

B. Arch Core (Verrucano), still capping the higher summits. The dark line represents the (middle limb) (squeezed out) between the arch and trough cores, all the rocks from Permian to Jurassic being reduced to the Lochseitin Kalk.

C. Trough Core (highly contorted Eocene shales).

D and E. Trough Limb (Mesozoic rocks and Verrucano).

The same conditions are repeated for the Southern Fold.

map, has done much in support of the position taken up by Heim.

In two countries especially the influence of the new views early made itself felt: in Scotland, by throwing fresh light on the complicated questions which had arisen, and in Belgium, enabling Köhler to attack the difficult problems connected with the Westphalian Coalfields (8), wherein he explains these overthrusts as being due to the overturn of folds, on the same theoretical basis as that adopted by Heim for the Doppelfalte. The Scottish discussion, after its long period of stagnation, was now re-opened, first by Drs.

Hicks and Callaway, but principally by the work of Professor Lapworth, who, in a series of papers on the Girvan succession, and the "Secrets of the Highlands," showed the marked influence Heim's work had had upon his own views, the publication of these articles having also the effect of inaugurating the application of the new principles to the problems and difficulties connected with the tectonic structure of our own island. What, then, were the results of these contributions from a tectonic point of view?

Undoubtedly, the principal results were: The recognition of the importance of isoclinal folding in the Ayrshire Palæozoic rocks; the discovery that zones, when carefully traced out, showed the actual presence of enormous folds; and that the repetition of the same fauna in upwardly ascending series was the result of the close parallel packing of a group of anticlinal and synclinal folds. The outcome of these labours has also served to overthrow the complicated conception of "Colonies" introduced by Barrande to account for the peculiarities observed in the palæontological succession.

Having thus far successfully endeavoured to elucidate these varied and difficult problems, Professor Lapworth next turned his attention to mountain structure, and was led to the conclusion that folding was the result of lateral compression. He observes: "At the foot of a mountain range the inward thrust and the outward counter-thrust are approximately equal in amount and opposite in direction, and the resulting folds are normal and regular (normal or amphiplexal folds), but as we proceed towards the centre of the range, whilst the thrust inwards remains approximately the same, the counter-thrust outwards is aided by the effect of the gravity of the mass above, and these two unequal forces are applied to the stratum obliquely with respect to each other. As a natural consequence, the axes of the rock folds no longer remain vertical, but *slope obliquely outwards, i.e.*, in that special direction in which the folding and ascending strata encountered the least resistance to their extension" (*Geol. Mag.*, p. 198).

In the discussion on overfolding, Professor Lapworth practically recognised every stage; from the condition where no separation of arch limb, middle limb, and trough limb had taken place, and where the middle limb has been rolled out between the arch core and trough core (this being Heim's theory for the Doppelfalte), to that wherein dislocation actually ensues; in which case the arch portion travels in a rigid mass over the trough. It will therefore be seen that Lapworth practically adopted Heim's conclusions, with all their far-reaching hypotheses.

Applying these to the Sutherland area, he concluded that, far from dealing with a simple succession, the Upper Gneiss represented only a part of the Sutherland gneisses that had undergone one of these tremendous overthrusts, the rocks taking part in the movement having suffered great deformation and contortion, thus giving rise to those sheared and pseudo-foliated structures, which he has included under the term "Mylonitic".

Almost simultaneously Dr. Callaway came to a similar conclusion, whilst Dr. Hicks announced that the highest beds were, in fact, parts of the oldest rocks occurring in a broken anticlinal, and Professor Bonney likewise showed that the coarsest gneiss had been crushed into a perfectly schistose rock.

Powerful as were the facts and arguments brought forward by these various observers in weakening the position of the Murchisonian hypothesis, its final overthrow was only achieved by the publication of Sir Archibald Geikie's letter, and the first summary on the survey work of the Durness-Eriboll area (9), and thus, by the frank adhesion of the Geological Survey, was closed the excited controversy, the views as originally propounded by Professor Nicol, and rejected during so many years, being to a great extent triumphantly vindicated.

It was now recognised that these Scottish rocks, first commencing as gentle foldings, steepened gradually on the western front until, bending over, they became disrupted, and the eastern limb pushed forward. Reversed faultings

still further complicated the discussion, but special stress was laid on the thrust planes, which were regarded as reversed faults of very low hade. It is along such planes that the rocks in Durness have been thrust a distance of over ten miles. Here had taken place the re-arrangement of the mineral particles, giving rise to the crystalline schists already referred to, whilst at the same time the vertical worm tubes in the quartzites underlying the thrust plane have been flattened, drawn out, and bent over in a direction perpendicular to the strike of the same (*Nature*, 1884, pp. 29-35).

The final result of the great contest was hailed on all hands with the liveliest satisfaction. Professors Lapworth, in the *Geological Magazine*, Bonney, at the Geological Society, and Judd, in his presidential address before the British Association, 1885 (wherein he specially recalls the important services rendered by Nicol in the controversy), all recognised the importance of the new departure. The first two of these observers had already taken part in the fray, whilst the latter, after a visit to the ground, recognised the truth of Nicol's position, and in 1877 had urged him to re-open the whole question, but unavailingly.

Younger students can now follow with interest the varied phases and details of this battle of giants, and, whilst learning many a valuable lesson from their failures and defeats, may profit by their discoveries and their triumphs.

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(*To be continued.*)